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THE RELATIVE VALUE OF TIDAL AND UPLAND WATERS IN MAINTAINING RIVERS, ESTUARIES, AND HARBORS.

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I.

The two natural agencies which tend to keep clear the channels of tidal rivers and estuaries are: the tidal flow, *i. e.*, the run of the tide, passing up and down such channels twice in about every twenty-four hours; and the low-water flow, *i. e.*, the current, mainly of fresh water, passing through such channels at the end of each ebb tide, and up to the beginning of the next flood. The object of this paper is to investigate the relative value of these two agencies, in preserving the full depth and section of such channels.

The determination of this relative value is a matter of very great moment in many practical questions of marine engineering, and much discussion has taken place upon it. The reports for 1845, 1846 and 1847 of the Commissioners on Tidal Harbors in particular, contain many opinions, and a considerable amount of information, bearing on the subject, but they cannot be said to furnish a solution of the problem.

One preliminary remark must be made, viz., that no universal rule can be laid down, and that each case must be judged on its own merits. This remark is so much an accepted dictum in matters of engineering, especially marine engineering, that it may appear superfluous. But in this particular instance a universal rule has been laid down and supported by engineers of eminence, that no encroachment on tidal waters should ever be permitted; in other words, that nothing should ever be done to diminish the amount of tidal water flowing up and down a channel. The object of this rule is to prevent the silting up of the channel, and its ground can only be the assumption that the main, if not the only, cause which prevents this silting up is the tidal flow. In the author's opinion, this assumption cannot be maintained, and grounds quite as solid might even be found for exactly the opposite rule, viz., that tidal water should be wholly excluded from a channel, wherever possible. As already stated, the author declines to lay down any universal rule; but his study of the subject has led him to accept as a principle, that the main and essential agent which keeps clear the channels of tidal rivers is not, in general, the tidal, but the low-water flow.

This principle may be supported by the following line of argument:

(I.) The silt, which tends to choke up tidal channels, is almost wholly due to the tidal water, and not to the fresh water.

(II.) The tidal water brings up more silt on the flow than it takes down on the ebb; *i. e.*, on the whole, it tends to choke the channel, not to scour it.

(III.) The low-water flow, if left to itself, scours away the deposit and keeps the channel open.

(IV.) Therefore, where the two act together, the scour must be due mainly, if not entirely, to the low-water flow, and not to the tidal flow.

(I.) The silt, which tends to choke up tidal channels, is almost wholly due to the tidal water, and not to the fresh water. In some quarters the idea still seems to prevail that the mud of tidal rivers is all brought down by those rivers themselves. But if this were true, then rivers, above the tidal area, would carry as much silt as below; and since they are there much smaller, the amount of silt per cubic foot, or the muddiness of the water, would be greater. The contrary is notoriously the case. Let any one compare the current of the Severn at Worcester with the immense mass of turbid water which occupies the Bristol Channel; or even the Thames at Richmond with the Thames at Erith. A good example on this head is furnished by the river Parret, at Bridgewater. This river is formed by several streams, which spring from the sandstone and limestone hills of Somerset, and unite in a great level plain many miles above Bridgewater. Any small amount of silt which they might wash down in their earlier course, would be deposited in this plain long before it could reach Bridgewater. But above and below that town the banks are regularly cut into level benches, for the sake of collecting the silt, which is eventually made into what is known as "Bath brick." The silt is deposited on these benches with great rapidity, requiring to be excavated every two or three months, and many thousands of tons are thus abstracted from the river annually, an amount which would be utterly beyond the capacity of the river itself to supply in that limited area.

To complete the proof, it is only necessary to answer the question, Where does this silt come from? In the case last described, this is an easy matter. At Stert Point, close to where the Parret debouches into the Severn, the river can be seen washing and undermining, for a considerable distance, a vertical bank of sandy clay, exactly resembling the silt which it deposits higher up. In other cases, such as the Severn, the silt is due to the constant motion of the waves beating upon the wide muddy flats which lie between high and low water. The mud on those flats, originally derived from the eating away of alluvial lands near the estuary, combined with a small portion of mud brought down by the river from inland, has been rising and settling alternately under the variations of weather, probably for centuries.

The effect of waves, even of small ripples, in stirring up mud, has been well illustrated in a paper by Mr. Charles Richardson, M. Inst. C. E., read before the British Association, at Bristol, in 1875. Speaking of the Avon between Bristol and Avonmouth, he says: "At the ferry from Pill to Lamplighters, the Lamplighters shore is entirely alluvial, and it will be found that during a quiet season the mud on that shore will rise to a level of 5 or 6 feet, on both sides, above the level of the ferry path. If, after this, a continuance of strong westerly winds sets in, the ripple on the water will gradually wash away the accumulated mud again, until it becomes almost level with the ferry path. This fact proves that the tidal waters do of themselves have a tendency to deposit mud, but that the ripple, caused by the wind, washes it away again in exposed situations near the mouth of the river."

(II.) The tidal water brings up more silt on the flow than it takes down on the ebb; *i. e.*, on the whole, it tends to choke the channel, not to scour it.—That tidal waters do bring up the silt from the large estuaries, where it is raised by wave action, into river channels, is made clear by section (I). That this silt is rapidly deposited at the bottom, during the period of slack water at the top of the tide, is an unquestioned fact, and follows from the laws of the mechanical suspension of solids in water. That a certain quantity of the silt so deposited is removed again during the run of the ebb, and carried out to sea, may also be granted. The question is, whether the whole, or only part, of the silt is so removed. If the latter, then a certain quantity is left behind after each tide, and unless some other agency super-

venes, the channel will gradually but surely silt up. Now, that only part and not the whole of the silt is so removed may be proved, in the author's opinion, as follows:

(1) To ascertain the effect of any cause, the best course is to examine cases where that cause alone is operative. This, in the matter of tidal scour can fortunately be done. The banks of any tidal and muddy river, between high and low water, are fully exposed to the action of the tidal scour, and to that alone. It is well known that these banks are generally covered, from top to bottom with a thick deposit of silt. This deposit has certainly been brought up by the flood tides, and as certainly has not been swept away by the ebb. Here, therefore, is a proof that tidal waters cannot sweep away as much mud as they deposit.

(2) It might be argued that this only applies to the upper portions of these banks, which, from the rapid fall of the tide, are not so long exposed to its influence. But, apart from other considerations to be subsequently mentioned, not only does the deposit retain about the same thickness from the top to the bottom of the slope, but in muddy waters the rise and fall of a single tide is sufficient to leave a distinct film of silt over the whole surface of that slope. For instance, during tidal work at or near low water in the Avon near Bristol, the planks, wheelbarrows, &c., had always to be washed before commencing operations at each ebb tide.

(3) It may be asked how it happens, if this deposit is always going on, that the channel is not finally choked up. The answer is that the deposit does accumulate, until the angle at which it lies becomes greater than the angle of stability of this soft and wet material; a portion then slides down into mid-channel, to be carried away by the low-water flow, and the accumulation commences afresh. This is well described in Mr. Richardson's paper already alluded to, as follows:

"Now, if the river channel be carefully observed at low water of a spring tide, flakes of mud may be observed to slip down into the low-water stream here and there as the tide recedes; the hollows these mud-slips leave are characteristic, and are plainly observable all along the river at low water. These patches of mud slip into the low-water channel as they lose the support of the tidal water and are then carried out into the Severn by the stream of fresh water. This process is constantly going on, and is evidently the way in which the channel is kept at its regular width, notwithstanding the increasing deposit of mud by the tidal waters at every tide."

(4) It is known (see Bouniceau, "Etude sur la navigation des rivières à marées"), that the amount of deposit is always less in winter than in summer. Since the tidal flow is the same in winter and in summer, this can only be due to the deposit being swept away by the winter freshes; and it therefore shows that the deposit diminishes as the scouring power of the low-water flow increases.

(5) Hitherto the question has perhaps been complicated by the low water and the tidal flow being both present in the same river channels. But it is easy to find examples where the former is entirely absent. In all such, so far as the author is aware, the result is the same, viz., that complete silting up does actually occur, unless artificially prevented. Every half-tide basin is a case in point, and it is well known that in muddy waters these have always to be kept to the proper depth by artificial means. Two special cases of a more general character may be given as further illustrations.

The first refers to the mode in which the Avon debouches into the deep water of the Severn at King Road. In former years the fresh water was discharged through two channels, embracing between them Dumball Island, and named respectively the Swash and the North Channel. The latter was the larger, and was chiefly used by the shipping. When the Bristol Port and Pier railway was made from Bristol to Avonmouth, two timber jetties, with landing-places and pontoons, were constructed in the North Channel, to enable goods and passengers to be landed for conveyance by the railway. About twenty years ago, however, the tugs and other steamers plying between Bristol and places down the Bristol Channel found that they saved time, as well as distance, by taking the Southern or Swash Channel, whenever the tide was sufficiently high to admit

of it. The passage of these steamers, often almost scraping the bottom, gradually deepened the Swash Channel, until the whole of the low-water flow was diverted down it. From that moment the North Channel, though still exposed to the run of the ebb-tide down the Avon and the Severn, began to slit up; and so rapidly has this process gone on that no vessels can now use this entrance, the landing places and jetties have been completely abandoned; and Dumball Island bids fair, in a few years, to be connected with the mainland by an expanse of mud only overflowed at high tides.

The second case is that of the Chesel Pill, near New Passage on the Severn. A "pill" in this district means a tidal creek, into which are discharged, through a sluice, the waters of an inland watercourse or "rhine." A plan of the Chesel Pill is given in Fig. 3. The sluice originally stood at Redwick, about a mile inland, and the channel below this was tidal. Some years ago it was resolved, for the benefit of the level, to dam the stream nearer to its mouth. For this purpose a new sluice D was built within the horse-shoe bend ACB, and a straight cut was made from A to B. The original course was then dammed at B, and the new cut opened, causing the water to pass through the new sluice D. The results have been as follows: (1) Above the sluice D the high banks, which were formerly covered by the tidal waters only, have been overgrown with grass, the fresh-water channel remaining the same; (2) Below the sluice D the whole channel remains as deep as ever, in spite of its having lost the scour of all the tidal water which formerly ran up to the old sluice; (3) The horse-shoe ACB has completely silted up, although it has had the daily scour of the tide running up to B.

These two cases have been dwelt upon as peculiarly instructive; but ordinary experience and observation in such matters are enough to teach that a creek, or any other space, left exposed simply to the flux and reflux of the tide, will in time silt up to something like high-water level; and this can only be because tidal waters tend on the whole to choke and not to scour.

(III.)—That the low-water flow, if left to itself, scours away the deposit and keeps the channel open, is evident from the remarks already made as to the Chesel Pill, which forms a salient instance of the truth of this statement. Further, ordinary rivers, in parts above tidal influences, keep, with slight oscillations, a constant regimen, and require no artificial means to maintain their channels. The only exceptions are where rivers, previously charged with silt, descend into wide and level plains, such as those of Lombardy. But these cases rarely occur in tidal waters, where the fall practically coincides with the slope of the land, towards the sea, and is seldom very small. As confirmatory of this it may be mentioned that even in tidal waters the bottom is kept clean by the low-water flow. This has been proved in the Avon, where, in the course of the improvements, it was desired to deepen the channel for some distance below the Clifton Suspension Bridge. Although the tidal banks are here covered with mud to the depth of some feet, and the water is always thickly charged with sediment, the spoil, brought up by the dredger, proved to be entirely composed of gravel and stones, so clean that it only needed the breaking up of the larger pebbles, and the addition of lime or cement, to form excellent concrete. An exactly similar layer of clean gravel, mixed with bones, trees, &c., is found over the whole of the alluvial lands of the Avon, covered by about 25 feet of tidal silt. This must be the old bottom of the river, silted over as the stream gradually worked its way from one side of the valley to the other. What can be the cause of this phenomenon, unless the silt, which is certainly deposited after each tide (and most thickly in the deepest water), is regularly swept away by the low-water flow, in the period before the next tide begins?

The author has now, he believes, succeeded in establishing the three propositions stated at the outset, as constituting the premises of his argument. If so, there appears to be no means of avoiding the conclusion, (IV.) that where the tidal and the low-water flow act together, the scour of the channel must be mainly due to the latter, and not to the former. A few subsidiary arguments may be added. The scour of a low-water flow is in its nature self-regulating. The velocity, and therefore the scouring power, of the water, de-

Fig. 1

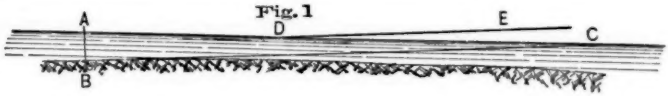
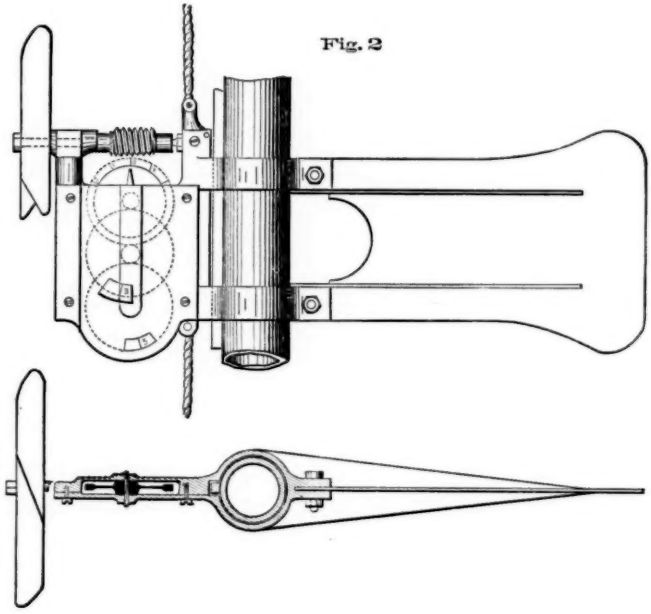
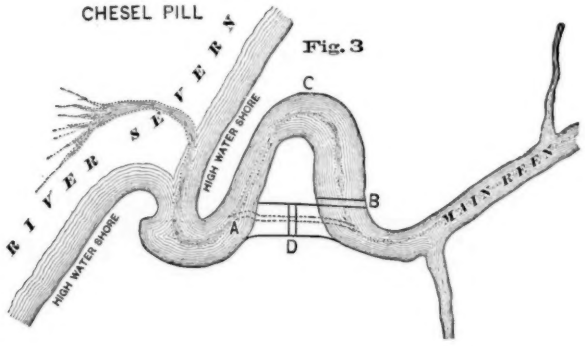


Fig. 2



Current Meter. General View and Plan. ¼ full size.

Fig. 3



pend on the slope of the bottom. If the bottom be anywhere too deeply cut into, this slope, from thence downward will be lessened; the velocity will thereby fall, and the bottom will silt up again. But the scour of tidal flow would seem to be of an opposite character. It is due to the flowing down of a mass of water, following the fall in the level of the ocean outside; and in proportion to the size of this mass, so apparently would be its scouring effect. Hence a tidal channel,

in which the amount scoured out once becomes larger than the amount deposited, should go on enlarging indefinitely. It is needless to say that such a case is unknown. On the contrary, there is good reason for the belief that, in alluvial soils, the section of a channel always bears a fixed relation to the amount of fresh water coming down. This is illustrated by the accompanying cross sections of the Avon, Fig. 4, Sections 1—5, token at different points between Bristol and King Road. It will be seen that the low-water stream occupies a similar position at the bottom of each section (becoming somewhat larger in the lower sections), and that the mud banks slope up from this at a regular angle, which is less at the sections near the mouth, because the mud there is softer and wetter. In this view the author is supported by Mr. Richardson, who has been a close observer for many years of such phenomena in the Bristol Channel, and who says: "If any one will look carefully at the different streams in the neighborhood, he will see little streams with little estuaries, medium streams with middle-sized estuaries, and big streams with large estuaries; the channel formed being always in proportion to the amount of fresh water. This cannot be accounted for by any other supposition than that the volume of the fresh water determines the size of the estuary, and that the tidal waters have nothing at all to do with it."*

But whatever may be the worth of the above argument as to the slight value of tidal, compared with low-water scour, it is desirable to check it, if possible, by direct experiments. Now, obviously, the scouring action of any current is due solely to the layers of water in immediate proximity to the sides and bottom; and the main body, flowing above, has no direct effect. This has been frequently ignored, and perhaps not unnaturally. If an observer stands on the brink of such a river as the Avon, and sees the ebb-tide rushing past, at a speed of sev-

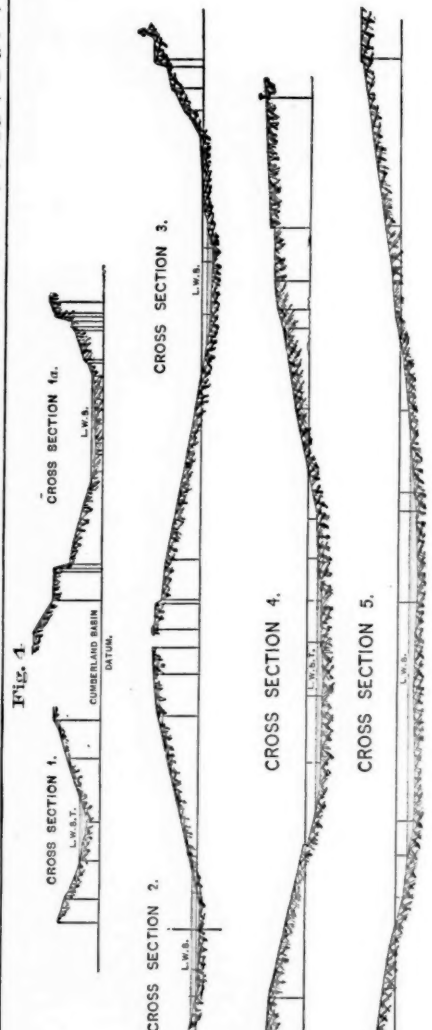


Fig. 4.

* It is obvious that another factor enter directly into the determination of the size of an estuary, viz., the size of the valley of which the estuary forms part; but as the size of the valley will as a rule govern the size of the stream which flows down it, the effects of the two factors are generally the same. Of course this does not hold when the valley has been excavated by ice or otherwise in solid rock, like the glens of Scotland or of Norway.—W. R B.

TABLE I.—GRAEVE'S AND SCHLICHTING'S OBSERVATIONS.

Reference No. of Experiment.	Depth.	Surface or Maximum Velocity.	Bottom Velocity.	Ratio of Bottom to Surface Velocity.	Mean.
	Meters.	Meter per second	Meter per second		
S. I.	1.4	0.636	0.4926	0.77	—
S. V.	1.8	0.551	0.3296	0.60	—
S. II.	1.9	0.714	0.4752	0.69	
20	2.0	0.79	0 50	0.64	
23	2.0	0.82	0.48	0.59	
22	2.0	0.75	0.43	0 56	
24	2.0	0.78	0.50	0.64	
29	2.0	0.83	0.46	0.55	0.61
33	2.1	0.786	0.46	0.585	
49	2.1	0.78	0.47	0.60	
21	2.1	0.79	0.52	0.66	
53	2.1	0.78	0.45	0.58	
28	2.1	0.84	0.53	0.63	
15	2.2	0.83	0.45	0.54	—
17	2.2	0.78	0.49	0.63	—
14	2.3	0.82	0.50	0.61	—
32	2.4	0.805	0 48	0.596	—
48	2 4	0.81	0.48	0.59	—
52	2.4	0.82	0.47	0.57	—
9	2.5	0.82	0.52	0.64	—
11	2.5	0.82	0.56	0.68	—
10	2.6	0.81	0.44	0.54	—
7	2.7	0.82	0 52	0.64	—
6	2.8	0.87	0.55	0.63	—
31	2.8	0.855	0.55	0.64	—
39	2.8	1.12	0.55	0.49	—
47	2.8	0.83	0.55	0.64	—
40	2.8	1.19	0.55	0.45	—
51	2.8	0.86	0.55	0.64	—
42	2.9	1.13	0.55	0.49	
1	2.9	0.88	0.55	0.57	
41	2.9	1.08	0.57	0.53	
44	2.9	1.19	0.67	0.56	
46	3.0	1.10	0.60	0.545	
36	3.0	1.18	0.70	0.59	
50	3.0	1.10	0.60	0.55	
43	3.0	1.16	0.60	0.52	0.55
54	3.0	1.10	0.60	0.54	
45	3.0	1.07	0 65	0.61	
35	3.1	1.14	0 62	0.54	
37	3.1	1.13	0.60	0.53	
2	3.1	0.89	0.50	0.56	
38	3.1	1 18	0.70	0.59	
35	3.1	1.14	0.57	0 50	

eral feet per second, he is easily led to the idea that an immense work of scouring must inevitably be going on. But a moment's reflection will show that it is only the velocity at the surface which he sees; and that unless he knows the ratio of this surface velocity to that at the bottom, he really knows nothing as to the intensity of the scour.

It is thus a matter of great importance to determine the ratio between the surface and the bottom velocity in tidal currents; and this point the author set himself to investigate. The data which he was able to discover were at first extremely meagre. While a multitude of experiments are recorded on the surface and middle velocities of rivers, yet, owing perhaps to the difficulties of measurement, the bottom velocities have been generally omitted, or at least have not been recorded with exactness.

Mr. W. Bald states that the bottom velocity of the tidal flow in the Clyde is about $\frac{3}{5}$ that at the surface, but unfortunately the depths are not mentioned. Rankine gives the ratio as $\frac{3}{5}$ for ordinary, or $\frac{1}{2}$ for very slow currents.

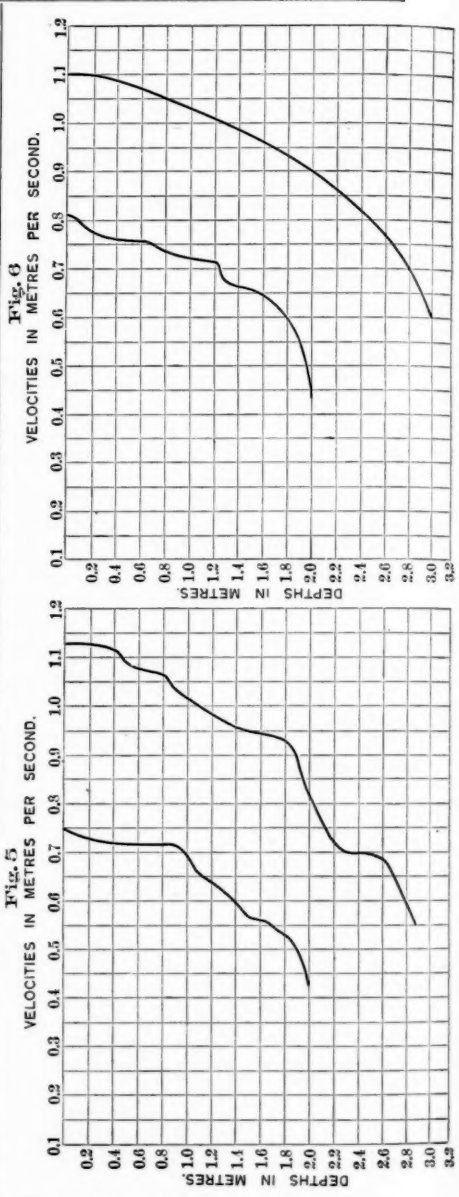
The best observations the author was at first able to meet with were those of

Graeve in the Oder and the Warthe, published in the "Civil Ingenieur." But these experiments were only in depths varying from about 2 to 3 meters (6 feet 7 inches to 9 feet 10 inches) while even in small rivers like the Avon the tidal current has a depth of more than 30 feet. The author, however, prepared the following Table I., showing, for gradually increasing depths, the surface velocity (or the maximum velocity where this was not exactly at the surface), the bottom velocity, and the ratio of the two, deduced from Graeve's experiments. The figures added for the depths of 1.4 meter, 1.8 meter, and 1.9 meter, are from Schlichting's observations on the Memel.

These results present considerable anomalies, especially in the figures for the depth of 2.8 meters. Taking, however, a mean of the observations from 1.9 meter to 2.1 meters, and also of the observations from 2.9 meters to 3.1 meters, it appears roughly that the ratio of the bottom to the surface velocity has diminished, between the depths of 2 meters and 3 meters (6 feet 7 inches and 9 feet 10 inches), from 0.61 to 0.55, or by about 10 per cent. It is clear that if anything like this rate of diminution goes on as the depth increases, the velocity at such depths as 20 to 30 feet must always be a mere fraction of that at the surface. This conclusion is confirmed by two experiments at such depths, given in Révy's "Hydraulics of Great Rivers." These, reduced to French measures for comparison, are as under:

Depth.	Surface Velocity	Bottom Velocity.	Ratio of Bottom to Surface Velocity.
Meters.	Meter per sec.	Meter per second	
6.9 (22 ft. 8 in.)	0.593	0.256	0.43
7.4 (24 ft. 4 in.)	0 549	0.178	0.32

It is to be observed, moreover, that such observations cannot be taken exactly at the bottom (Mr. Révy's were 1 foot from it), and that the diminution in velocity close to the bottom is very rapid, as is shown by Graeve's curves of velocities, at different sections, four of which are reproduced in Figs. 5 and 6.



The author's attention was subsequently directed, by Mr. Robert Gordon, M. Inst. C. E., to the Report of the Chief Engineer of the U. S. Army for 1878, which contains a valuable record of ex-

periments made by General Ellis in the Connecticut river, at a part where the width varied from 1,400 to 1,000 feet and the depth exceeded 20 feet. The velocities at the surface, and at various depths from thence to the bottom, were measured, partly by floats but chiefly by meter. With the meter, observations could be got as near to the bottom as 0.4 foot in some cases; with the floats they could not be nearly so close, and with these the results as to bottom velocities are so irregular that the author decided to reject them. The meter observations also presented several anomalous results, some in one direction, some in another, and these also have been rejected. The remainder are grouped, in Table II., into two classes, according as the surface velocity was above or below 2 feet per second; a division practically suggested by General Ellis, and justified by the result.

From this table it appears that for low velocities the results are too irregular to allow any rule to be formulated, but that the ratio of bottom to surface velocity is always small, varying on the average from 0.40 to 0.20, and in some cases falling as low as 0.03. In the higher velocities the results are far more regular; the average ratio for the observations at 7 feet and 8 feet is 0.61, and for those at 20 feet and 21 feet is 0.44, showing a diminution between those depths of 28 per cent. The latter ratio, 0.44, agrees closely with one of Mr. Révy's, given above, and the results as a whole certainly go to confirm the conclusion already formed.

Another series of experiments, to which the author was also directed by Mr. Gordon, are contained in the Report to the King of Holland on Dutch Public Works, for the year 1873. These experiments were carried out at several places in the various channels of the Rhine and Meuse. These channels, though not deep, are about 500 yards wide, and the results appear to indicate an intermediate position between first-class rivers, such as the Mississippi and the Irrawaddy, on the one hand, and ordinary rivers on the other. There is a considerable difference, generally ranging from 40 to 50 per cent., between the records of the surface and bottom velocities; but the ratio is irregular, and it does not decrease with increase of depth to the same extent shown by the experiments given in Tables I. and II. The detail of some of these experiments is given in Table III.

The above observations were all made in ordinary fresh-water rivers, and, in the author's opinion, would not fully represent the circumstances of the ebb in a tidal channel a short distance from the sea. An ordinary river in a condition of steady flow, may be taken for theoretical purposes as being of unlimited length, and a horizonal line, B C (Fig. 1), drawn through the bottom of any section A B, will cut the surface at some point, C, determined by the average fall of the river. The triangle of water, A B C may be considered as acted on by the full force due to the pressure on A B, and will move with a velocity such that the work done by this pressure is just sufficient to overcome the various resistances to the flow. At every point between B and C the conditions will be the same, and the water will be moving with the same steady motion. But the case of a tidal channel is different. There the water at some intermediate point, D, falls into an estuary or ocean, whose waters are either at rest, or at any rate are moving in a direction different from that of the current in the channel. The waters of the channel, below the level D E, can only make their way into this estuary by displacing the waters already there; and to do this must require some expenditure of energy, especially as the estuary waters will, in general, have the higher specific gravity of the two. The result must be a back pressure on the water at B, additional to the resistance of friction, &c., and, in consequence, a retardation of its velocity. This retardation or loss of energy will apparently be greater as the depth increases; and, therefore, near the bottom, where the speed is in all cases low, it would seem probable that, in tidal channels of this character, the velocity may be reduced to a very small amount, or may even disappear.

Influenced by these considerations the author became anxious to obtain some actual observations on the velocity at the bottom of such channels during an ebb tide. Some time elapsed before such an opportunity presented itself, but at length, in March, 1880, Mr. Thos. How-

TABLE II.—OBSERVATIONS ON THE CONNECTICUT RIVER.

Velocity above 2 feet per second.						Velocity below 2 feet per second.					
Depth.	No. of Experiment.	Surface velocity.	Bottom velocity.	Ratio.	Mean Ratio.	Depth.	No. of Experiment.	Surface velocity.	Bottom velocity.	Ratio.	Mean Ratio.
Ft.						Ft.					
7	34	3.88	2.22	0.57	0.65	7	—	—	—	—	—
	35	3 39	2.29	0.67							
	33	3.99	2.81	0.70							
	28	3.96	2.18	0.55							
	32	3.75	2.86	0.76							
8	26	3.68	1.83	0.50	0.54	8	48	1.03	0.50	0.49	0.44
	27	4.07	2.09	0.51			70	1.38	0.41	0.40	
	26	3.10	1.91	0.62							
9	—	—	—	—	—	9	81	1.48	0.30	0.20	0.20
							83	1.33	0 28	0.21	
10	29	4.05	2.00	0.49	0.53	10	23	1.61	0.53	0 33	0 33
	11	2 54	1 48	0.58							
11	—	—	—	—	—	11	—	—	—	—	—
12	10	2.65	1.34	0.51	0.51	12	54	1.06	0.46	0.43	0.35
							80	1.70	0.46	0.27	
13	—	—	—	—	—	13	39	1.27	C 04	0.03	0.21
							47	1.54	0.61	0.40	
14	79	2.10	1.34	0.59	0.59	14	14	1.29	0.25	0.19	0.29
							22	1.83	0.72	0.39	
15	21	2.14	1.09	0.51	0.51	15	59	1.23	0.64	0.52	0.52
16	2	3.12	1.43	0.46	0.48	16	46	1.66	0.56	0.34	0.28
	30	4 30	1.89	0.44			53	1.30	0.29	0.22	
	20	2.17	1.14	0.52							
	78	2.18	1.09	0.50							
17	8	3.36	1.58	0.47	0.47	17	18	1.57	0.73	0.47	0.47
18	77	2.49	0.97	0.39	0.39	18	17	1.60	0.46	0.29	0.29
19	—	—	—	—	—	19	15	1.49	0.36	0.24	0.39
							19	1.62	0.64	0.39	
							73	1.37	0.43	0.31	
							75	1.91	1.24	0 64	
20	44	2.54	1.07	0.42	0.42	20	—	—	—	—	—
21	42	3.03	1.43	0.47	0 45	21	—	—	—	—	—
	3	2.31	0 96	0.42							
	41	2.80	1.30	0.46							

ard, M. Inst. C. E., Docks Engineer to the corporation of Bristol, kindly placed men and materials at the author's disposal, and the necessary experiments were undertaken and carried out with much zeal and ability by Mr. H. S. Hele Shaw, Assoc. M. Inst. C. E., Whitworth and Miller scholar, and assistant to the Professor of Engineering, University College, Bristol. The observations were taken at a point, shown in Fig. 5 (Plate 1), on the "New Cut," at Bristol, be-

TABLE III.—EXPERIMENTS ON LOWER RHINE.

Width of river 460 meters; distance from bank to nearest section, 70 meters.

Date of Experiment.	Number of Section.	Depth.	Surface or Maximum Velocity.	Bottom Velocity.	Ratio of Bottom to Surface Velocity.	Mean.
		Meters.	Meter.	Meter.		
July 11	V.	2.6	1.095	0.70	0.64	
" 6	V.	2.85	1.085	0.65	0.61	
July 22	IV.	3.4	1.395	0.825	0.59	0.63
June 10	IV.	4.0	1.27	0.85	0.67	
July 15	V.	4.7	1.34	0.93	0.69	—
" 6	IV.	4.9	1.265	0.65	0.51	—
July 6	II.	5.4	1.71	1.20	0.70	—
July 22	III.	6.5	1.515	0.875	0 58	
" 6	III.	6.6	1.535	0.85	0.55	
June 30	III.	6.6	1.475	0.77	0.52	
July 15	III.	7.0	1.62	1.025	0.63	0.575
" 15	III.	7.2	1.725	0.95	0.55	
" 12	II.	7.5	1.775	1.10	0.62	

tween the Bathurst and Cumberland basins. This cut is nearly straight, and is well leveled at the bottom, being excavated in the New Red Sandstone rock; it is, therefore, eminently suited for such observations, as it is free from errors due to bends in the river, or to inequalities in the bottom. The observations were made by means of a current meter, kindly lent by Mr. Baldwin Latham, M. Inst. C. E. The observed velocities at the surface were checked by floats, and in almost all cases the meter gave the higher value, so that its error, if any, was one of excess. The bottom velocities were taken by means of a strong rod, let down perpendicularly from the boat to the bottom, and having a piece of sheet iron, 8 inches square, on the end to prevent its sinking in the mud.* The meter was passed down this rod as far as it would go, and was then allowed to run for a fixed time (five or three minutes). It was then stopped, pulled up to the surface, and the reading taken. The middle velocities were determined in the same manner. The results are contained in Table IV., and although, like all current observations, they present some anomalies, they will be found very instructive. The small difference, varying from 1 foot 1 inch to 2 feet 1 inch, between the surface level at Avonmouth (where the Avon debouches into the Bristol Channel, 7¼ miles lower down) and that at the place of observation, brings out strongly the difference already pointed out between this case and that of an ordinary river. It is only in the last series, G, that the ordinary conditions may be said to appear. Unfortunately, no levels were taken on the tide gauges after 1 P. M.; but, from the rapid increase in the difference between the Bristol and Avonmouth levels from 12.40 to 1 P. M., it may be conjectured that at 1.20 P. M. this difference would have amounted to at least 4 feet, while at the same time the depth at the place of observation had diminished to 7 feet. The conditions thus approximated to those of an ordinary river, and it will be seen that here and here alone, the bottom velocity becomes considerable, and its ratio to the surface velocity approaches the ratios given by Graeve for similar depths.

The bottom velocities shown in Table IV. are very small, and not in themselves sufficient to move even the finest silt. That in E3 was 2 feet from the bottom, and it will be seen from F3 and F4 that the ve-

* This was found practically to be needless, as the bottom was clean rock; a fact which confirms the previous assertion as to the power of low-water flow to keep the bottom clear.

TABLE IV.—EXPERIMENTS ON VELOCITIES OF THE EBB TIDE IN THE RIVER AVON, MARCH 31, 1880.

Ebb commenced about 10.25 A.M. Depth at high-water, about 23 feet 6 inches.

Series and Number.	Total Depth at Time of Experiment.	Position of Meter.	Time of commencing Experiment.	Duration of Experiment.	Distance run by Meter.	Velocity by Meter.	Surface Velocity by Surface Floats.	Level of Surface at Avonmouth below Surface.
	Ft. In.		H. M.	Minutes.	Feet.	Ft. per Second.	Ft. per Second.	Ft. Ins.
A 1	22 11	At surface	10 55	5	627	2.09	1.9	1 5
2		11 feet below	11 10	5	312	1.04	2.6	
3		1 foot from bottom	11 3	5	55	0.18	2.6	
B 1	21 5	At surface	11 17	5	902	3.01	3.0	2 1
2		9½ feet below	11 32	5	309	1.03	—	
3		1 foot from bottom	11 24	5	38	0.13	—	
C 1	18 4	At surface	11 40	5	932	3.11	3.1	1 1
2		8 feet below	11 55	5	905	3.02	—	
3		1 foot from bottom	11 47	5	39	0.13	—	
D 1	15 2	At surface	12 2	5	905	3.02	—	1 2
2		7 feet below	12 19	5	901	3.00	3.3	
3		1 foot from bottom	12 12	5	26	0.09	3.0	
E 1	12 9	At surface	12 25	3	699	3.89	3.3	1 10
2		6 feet below	12 35	3	564	3.09	3.3	
3		2 feet from bottom	12 38	3	244	1.36	3.3	
F 1	10 10	At surface	12 40	3	524*	2.92	3.3	1 11
2		5 feet below	12 56	3	614	3.41	3.3	
3		2 feet from bottom	12 45	3	300	1.67	3.3	
4		9 in. "	12 51	3	19	0.11	No observation.	
G 1	9 0	At surface	1 0	3	689	3.83	Do.	3 3
2		7 feet from bottom	1 5	3	528	2.94	Do.	
3		5 " "	1 10	3	524	2.91	Do.	
4		3 " "	1 16	3	514	2.86	Do.	
5	7 0	1 foot "	1 21	3	417	2.32	Do.	

locity at 2 feet may be considerable when that at 1 foot is practically zero. These velocities, however, are given only because they were actually recorded on the meter, and not as being absolutely correct. Preliminary experiments had shown that it was not possible satisfactorily to put the meter into gear while it was at the bottom, and, therefore, during these experiments, it was put into gear before being lowered into the water. The effect of course was that the meter was running during the time it took to descend through the current, and that the distance thus run must properly be subtracted from the distance indicated at the end of the experiment.† If this subtraction were made there could be little doubt but that the bottom velocity, already very small, would turn out to be absolutely nothing. This was confirmed by those who were conducting the experiments noticing that the small plate which has been mentioned as attached to the bottom of the rod, whenever it was lifted to the top of the water, was found to have a deposit of fine silt on its upper surface. This seems to prove that, at the very moment when an ebb tide is running down at a speed of 3 to 4 feet per second, not only may there be

* Probably there is an error in this reading.

† It will be noticed that in the later experiments, where the depth was less, the distance thus run was also less.

TABLE V.—EXPERIMENTS ON CURRENT VELOCITY IN THE AVON, SEPTEMBER 21, 1880.

Ebb commenced about 8.40 A.M. Depth at H.W. about 26 feet 5 inches.

Position of Meter.	Time.	Depth of Water.	Surface Velocity by Floats.	Velocity by Meter.
	H. M.	Feet. Inches.	Feet per Second.	Ft. per Second.
Surface	9 40	22 8	—	3.57
Bottom	9 50	21 0	4.1	0.00
6 feet from bottom	9 55	20 0	4.3	2.70
Bottom	10 10	17 4	4.5	0.00
Surface	10 15	16 10	4.6	4.60
Middle	10 21	16 0	—	3.40
2 feet from bottom	10 25	15 9	4.3	0.05
Middle	10 30	15 2	—	3.44
Surface	10 33	14 10	—	4.60
2 feet from bottom	10 36	14 6	—	0.00
4 feet from bottom	10 38½	14 0	—	1.00
Bottom	10 44	13 6	—	0.00
Middle	10 49	13 0	—	3.65
Surface	10 52	12 6	—	4.60
1 foot 6 inches from bottom	11 2	11 4	—	2.40
Middle	11 6	11 0	4.4	3.80
Surface	11 10	10 8	—	4.08
Bottom	11 13	10 4	—	3.30
Surface	11 16	10 0	4.6	4.04
Bottom	11 20	9 6	—	3.30
Middle	11 24	9 2	—	3.40
¼ depth	11 27	Not taken.	—	3.50
¾ depth	11 30	Do.	—	3.40
Surface	11 33	8 4	4.1	4.04
Bottom	11 37	8 0	—	3.35
Middle	11 40	7 8	—	3.50
¼ depth	11 43	7 4	—	3.87
¾ depth	11 46	7 2	—	3.40
Surface	11 50	7 0	3.6	3.90
Bottom	11 55	6 9	—	2.87
Middle	11 58	6 8	—	3.20
Surface	12 1	6 6	—	3.60
Bottom	12 8	6 4	3.4	2.70
Middle	12 11	6 0	—	3.00
Surface	12 18	5 10	—	3.20
Bottom	12 22	5 9	—	2.35
Middle	12 24	5 9	—	3.00
Surface	12 25	5 8	3.5	3.07
Bottom	12 34	5 7	—	1.91
Middle	12 37	5 6	—	2.60

no scour whatever going on at the bottom, but an actual deposit of silt may be taking place. It is obvious that the theoretical views already stated have met with the fullest confirmation.

In order, however, that this point might be put beyond all doubt, the author arranged with Mr. Shaw to make a fresh set of experiments with improved apparatus, and these were successfully carried out on the 21st of September, 1880. In this case, following an ingenious suggestion of Mr. Shaw, the rod on which the meter was mounted (Fig. 2) was made with a feather on it, which fitted into a recess in the socket of the meter. By this means the meter, which was specially constructed for the purpose by Mr. Shaw, could be turned in any direction with regard to the current by simply turning the rod. When being lowered through the water the meter was thus held in such a position that the screw had no tendency to revolve at all, a position which was found to be nearly, but not quite, at right angles to the direction of the current. When it reached the required depth (which was marked

upon the rod), the meter was shifted, by turning the rod, into the position parallel to the current, in which it was maintained by the vane; and it then began immediately to revolve. Its scale of measurement was carefully ascertained by preliminary trials, in comparison with floats—a ship's log, which was first used for the purpose, not proving sufficiently accurate; and it was so carefully adjusted and lubricated that the slowest current was sufficient to cause it to rotate. Under these conditions the experiments were carried out, and their results are given in Table V.

On this occasion the depth of high water at the place of observation was about 26 feet 5 inches, and the ebb tide began at about 8.40 A. M. The first observation was taken at 9.40 A. M.; the ebb had then been running for about one hour, and had lowered the surface level by about 3 feet 9 inches, or to a depth of 22 feet 8 inches. The first observation showed that the conclusion drawn from the former set of experiments, viz., that there was absolute stillness at the bottom, was correct, for while the velocity at the surface was 3.57 feet per second, that at the bottom was zero. The observations were repeated with the same result, till about 11 A. M., when the ebb had been running about two and a-half hours, and the depth had fallen to about 11 feet, or 15 feet below high water level. The bottom layers of water then appeared to start into activity, and to assume a velocity which from that period continued to bear a tolerably uniform ratio of about 0.7 to the surface velocity. The experiments were ended at 12.37 P. M., when the whole of the ebb was over, four hours after it had commenced, and one hour and a-half after the bottom velocity had become apparent. The depth was then 5 feet 6 inches, and the velocity, 1 foot from the bottom, about two-thirds that at the surface, a result which agrees very fairly with the conditions for an ordinary river, as determined by Graeve's and Ellis's observations.

The foregoing experiments, combined with others, appear to justify the laying down of the following rules as to the relation between bottom and surface velocities in various cases.

A. In the largest rivers the bottom velocity may for the present purpose be taken as the same as the surface velocity, varying within small limits only. This seems to be fully established by the observations of Humphreys and Abbott on the Mississippi, and more recently by the numerous and careful observations of Mr. Robert Gordon on the Irrawaddy, some of the MS. records of which he has most kindly placed at the author's disposal. In such cases the whole mass of water appears to move almost like a solid body, independently of the resistances of the sides and bottom. It is possible that the same may hold of great tidal estuaries, such as that of the Mersey, but on this the author offers no opinion, as he is unable to find any observations of bottom velocities made under such circumstances.

B. In rivers of ordinary size, such as the Connecticut and Memel, the bottom velocity bears to the surface velocity a ratio which diminishes as the depth increases, but with too much irregularity to allow of a formula being constructed. Roughly, the ratio may be said to be about three-fourths at a depth of 5 feet, one half at 15 feet, and probably one-third at 25 feet.

C. In ordinary tidal channels, such as the Avon below Bristol, the course of events during an ebb seems to be as follows: At first the slope of the surface is exceedingly small (in the Avon it was about 1½ foot in 7¼ miles), and, while the velocity at the surface is considerable, it diminishes rapidly from thence downwards, and at some distance from the bottom becomes *nil*. This continues for about two-thirds of the ebb, the surface velocity increasing up to a certain point, and then becoming nearly constant. During all this time, not only is no scour going on at the bottom, but, if the water be muddy, an actual deposition of silt is taking place. At this time, after about two-thirds of the ebb, the water has fallen about three-quarters of its total height, the slope of the surface has considerably increased, and the conditions approximate to those of an ordinary river. The bottom layers of the water then spring suddenly into motion, and the ratio of bottom to surface velocity is from thenceforward approximately as in the last paragraph, the surface velocity diminishing steadily as the tidal waters dis-

appear, until it assumes the normal rate of the low-water flow. During this period a scour of the bottom is of course going on; but as the velocity is not much higher than in the subsequent period of low-water flow, the rate of scour will not be much greater, and the actual scour will be insufficient to compensate for the amount of deposit from the tidal waters which has taken place, not only during the period of high water, but also during the first two-thirds of the ebb. It must follow, therefore, that the scouring effect of the ebb tide is little or nothing, and the observed incapacity of tidal flows to sweep away the silt they have deposited is amply and satisfactorily explained.

The general investigation as to the relative value of tidal and low-water scour on river channels may now be considered as concluded. It was begun by adducing numerous facts to establish the proposition that the effect of tidal flow was to cause channels to silt up, while the effect of low-water flow was to keep them open. It was then pointed out that the former circumstance could only be explained by the hypothesis that the current at the bottom of such tidal channels (the only current which has any scouring effect) was very slow, even when the surface current might be rapid. Observations were quoted to show that in ordinary rivers this diminution of velocity does exist to a large extent, and that it increases rapidly with increased depth. Theoretical reasons were then given for believing that in tidal channels this reduction of velocity would again be much larger than in fresh-water rivers, otherwise similar. Lastly, it was shown that this view was amply borne out by direct experiments, which have demonstrated that in some cases, at least, the bottom velocity is not only reduced, but destroyed, and that silt may be settling on the bottom when the speed of the surface current is at its highest.

To complete the subject, however, it must be observed that there are certain circumstances in which the passage of tidal waters through a channel may have some effect in keeping it open. These are, briefly, any circumstances in which the tidal waters are so far kept back on the ebb, that a portion of them remain to increase the velocity of the low-water flow. Thus a large tidal basin may be supposed to be connected with the sea by a very narrow sluice. Then the flow through the sluice would not be able either to fill or to empty the basin with sufficient rapidity to keep pace with the variation of level outside; hence, on the ebb, it would be low water outside for some time before the current would cease to run through the sluice; and the waters thus impounded would go practically to increase the volume of the low-water flow, and no doubt to add to its scouring effect. This is probably a very rare case. A common one is that of a harbor having a narrow entrance, but expanding inside into a wide extent of mud flats. A large quantity of water soaks into these flats during flood tide, when they are covered with the sea; and when the tide has ebbed this water oozes out again, not immediately, but gradually, and, winding through innumerable depressions and channels, finds its way to the main entrance. It thus forms, as it were, a spurious low-water flow, which, if the inland waters entering the harbor are small, may have a decided effect in preserving the depth of this main entrance. In the author's belief, it is in a great measure from the observation of such cases, which are common in some localities but rare in others, that the strong predilection in favor of tidal scour has taken its rise.

It remains to inquire how far these views agree with the facts and opinions adduced by various authorities on the question, and also to consider briefly their practical effect on some of the problems of marine engineering.

It has been already mentioned that the chief discussion of the subject, as far as English literature goes, is to be found in the reports of the Tidal Harbor Commissioners for 1845, 1846 and 1847. From these reports may be quoted the evidence of Mr. James Walker, Captain Washington, Sir John Rennie, Mr. D. Stevenson, Mr. Scott Russell, and others, as strongly insisting on the value of tidal scour, and deprecating any interference with tidal areas under any circumstances. But the value of these opinions is somewhat lessened on account of the very meagre substratum of facts on which they appear to be based. Thus special reference is made to Southwold as a case where great injury was done to a harbor by embank-

ing lands inside, and so diminishing the amount of the tidal flow. But from Mr. Walker's evidence the true nature of the injury appears to be this: The action of the tides on the East coast, as is well known, is to keep a bank of shingle continually traveling parallel to the shore, at a certain depth below high water. So long as the harbor in question was kept open to its full extent, the stream issuing from thence on the ebb was sufficient to sweep away the encroaching shingle as fast as it advanced, and so to preserve a clear entrance. After the embanking this was no longer the case; the shingle (aided, according to Mr. Ellis, by a strong easterly gale) pushed forward in spite of the current, formed a bar across the tideway of the basin, and thus made it comparatively inaccessible. This statement indicates a most peculiar case, which must be treated on its own merits, but must not for a moment be allowed to have any weight on the general question of the relative value of tidal and low-water flow.* Again, in the case of Rye, Sir John Rennie states, that the harbor was greatly deepened by the blowing up of a sluice, which enabled the tide to run further up the country. But it appears that the failure of the sluice was due to a heavy land fresh; and it seems pertinent to ask, whether the deepening of the harbor might not possibly be due to the land fresh also. In its general history Rye appears almost a typical case of a harbor gradually silting up from the deposit of tidal mud, the fresh-water flow being insufficient to keep it open. Beyond the evidence, such as it is, of these two cases, the author has been able to find little but simple expressions of opinion, and general axioms, of a more or less doubtful character.

Nor is the weight of opinion entirely on one side. Mr. William Cubitt, while disposed to attach considerable value to tidal scour, is most emphatic in pointing out that there are no principles of universal application to harbor engineering, and that there may be many cases where to embank lands would not be injurious, and to open them to the sea would not be beneficial. Mr. John Murray and Mr. W. C. Mylne, in their reports recommending the "dockizing" of the River Wear, speak of the excellent results achieved in France by sluicing from back-water reservoirs, or, in other words, by forming an artificial low-water flow. This is confirmed by Sir John Macneill in his report on Hartlepool Harbor, where he points out that the channel was kept open by sluicing, while it had silted up when left to the operation of the tide. To this testimony may be added that of perhaps the earliest of English marine engineers.

Smeaton's report on Wells harbor is an admirable account of what may be considered almost a typical case of the effects of scour. This harbor receives practically no inland water, but has a narrow entrance, and a considerable extent of marshy flats inside, through which the tide used formerly to find its way by small creeks and channels. Smeaton's report shows that the harbor has always been in a state of slow decay from the deposit of silt; but that the entrance was kept partially clear by the tidal water, which, oozing among these creeks and channels, gradually found its way back to the ocean; that, to improve this scour, a sluice, or rather a dam with one narrow opening, was erected to form a back-water reservoir; that this, for the whole time it lasted, proved efficient in keeping the harbor open; that, when it fell into decay, the authorities, instead of repairing it, chose to expend their money on a lawsuit with certain parties who had embank a portion of the marsh lands; and that the natural consequence of this policy was the destruction of the harbor. In commenting upon these circumstances, he makes the following remarks, which in the main coincide with the author's views, and are well worth careful consideration:

"The reason why the waters passing the sluice have a greater effect in scouring than those which return to sea without passing the sluice, is, because by the contracted opening of the passage of the sluice, the waters that lie in the creeks behind it are detained from ebbing so quickly as they otherwise would have done; that is, their numerous mouths when always open, reduced the level of the water contained therein, to nearly the

* It appears that there was always shoal water outside the harbor entrance, and that the shingle bar formed across the top of this shoal. If so, this would bring the advancing shingle within the range of the surface-current of the ebb, and enable that current to have its proper scouring effect.—W. R. B.

same level as that of the water in the main channel of the harbor, being stopped by dams made across and united by cross passages into one, and the mouth of this being contracted by the work called the sluice, a body of water is held back in these creeks, as reservoirs, which not being able to escape so fast as the tide ebbs in the main channel, it follows, that a body of water by these means is vended upon, and after the half ebb, which discharging itself into the harbor's creek, forms a scour when the depth is so much lessened as to operate with power in grinding the bottom, which otherwise would have been so languid as not to have stirred a grain of sand or mud, in which case its effect would be little or nothing. This artificial scour thus procured, in some degree imitates the effect of a fresh-water river, which in these situations is very greatly beneficial, not from any virtue there is in fresh water preferable to salt in these cases (if anything rather less on account of its less specific weight), but from its having a fall from the land, and proceeding therefrom continually it not only strengthens the ebb, but running to sea at low water when the fall being greatest, and the sandy bottom exposed to its action, it continues to work at a time when it can operate to the best advantage; and when the ordinary current of a river is assisted by extraordinary land floods and freshes from downfalls of rain and snow, and this operating at low water, when, as just remarked, the fall is the greatest, in such cases it is capable of producing extraordinary effects, and of keeping a harbor continually open with a channel of a given magnitude, though loaded with sands in any possible degree; for a fresh-water river has this peculiar advantage, that at the same time that it strengthens the scouring power of the ebb, it operates most forcibly at low water when there is the least to obstruct its operation; it opposes the tide of flood from the sea, and thereby prevents its bringing so much sand and silt into the harbor as otherwise it would.

"The defect therefore of this sluice of Wells is, that though it retains the waters so as to be behind the general ebb, and thereby strengthens the latter part of it considerably; yet being at low water all spent, when the greatest good might otherwise be obtained, it loses that good effect which would be had from a fresh-water river, or from a proper sluice; that is, one that will retain the water wholly till a proper time of tide, and then being let go in one collected body, is capable in a short space of time of producing marvelous effects."

Lastly, the author may venture to cite, in support of his views, the following passage from the important work of Bouniceau ("Etude sur la navigation des rivières à marées," 1845, p. 68). After speaking of the way in which estuaries become charged with silt, especially after storms, he continues: "Deposits are thus formed throughout almost the whole tidal course of the river; the ebb carries back only a part of these deposits, and the next tide, passing over the remainder, takes up a part, and deposits it higher up the river; so that the finest silt is borne to a great distance from the mouth. Each tide thus augments the quantity of the deposits thus formed, until a fresh in the river scours away the whole of this alluvium, bears it to the sea, and deposits it outside the mouth. It follows that, when the river is low, the depositing power of the flood tide exceeds the scouring power of the ebb, aided by the fresh water, and by the natural slope of the channel; during freshes, on the contrary, the latter power exceeds the former. It will be seen, therefore, that the ebb would not alone be able to keep open the entrance of a river, did not considerable freshes occur from time to time to increase its effect. On our coasts the ebb only serves partially to maintain the channel, until the winter comes and re-establishes it, scouring the bed, and enabling it to form in the next summer a vast reservoir for scouring purposes. If the fresh waters, and mainly the freshes, did not thus lend their aid to the ebb, if, from any cause, their flow was interrupted, rivers whose estuaries are charged with mud would be soon silted up by the action of the sea, and before long would be turned into cultivable land. A striking example of this effect has been given by the Somme. A lateral canal had been constructed beside this river from Abbeville to St. Valery. As this was large enough to convey the whole of the waters, it was decided to dam the channel near Abbe-

ville, and thus direct the fresh waters from their accustomed channel. The silting up of the bed of the Somme was the almost immediate consequence of this alteration."

Finally there has to be considered briefly the practical application of the principles brought out in the course of this inquiry. These principles may be stated as follows: The scouring power of any current depends wholly on its velocity at the bottom. This bottom velocity, except in rivers of the largest size, is much less than that at the surface, and in a ratio which decreases rapidly as the depth increases. In the case of a deep tidal channel not far from the mouth, it appears to be absolutely *nil* for at least the greater part of the ebb. Such channels, if muddy, are never kept open by the operation of tidal scour alone. Hence, in general, excluding the largest rivers and estuaries, the maintenance of the channel is mainly, if not entirely, owing to the low-water flow.

This flow is, of course, chiefly due to the fresh waters of the rivers: but the rivers are swollen slightly, perhaps, in all cases, and considerably in some, by the salt waters which continue to ooze out from the muddy banks or flats when not covered by the tide. This addition to the fresh waters will, of course, make the depth at any point, and therefore the slope and the velocity, greater than it would be if the fresh water acted alone. This greater velocity will no doubt give a certain advantage in scouring power. But the extent of this advantage cannot be estimated offhand. Theoretically, however, it may be roughly gauged as follows: Take the section whose bottom is exactly at the same level as the surface of the water outside the mouth—a level which will obviously be the same whatever the discharge of the river may be. Then the depth at this section will be proportional to the slope. Let this depth be H, and the mean velocity V. Then, by the ordinary principles of hydraulics, $V^2 = CH$, where C is a constant. Suppose that it be ascertained by measurements that half of this depth H is due to salt water, and now consider the effect of diminishing the area covered by the tide to such an extent that half of the salt water, which at present goes to swell the low-water flow, is subtracted. Believers in tidal scour would no doubt assert that the scour, in other words, the bottom velocity, would be reduced by one-half also. To see how far this would be the case, let h be the new depth, v the new velocity. Then, since the discharge is only $\frac{3}{4}$ of what it was before,

$$v\,h = 0.75\ V\,H.$$

But $h = \frac{v^2}{C}$, and $H = \frac{V^2}{C}$; hence, $v^3 = 0.75\ V^3$, $v = 0.91\ V$; in other words, the mean velocity will be diminished, not by 50 per cent, but by 9 per cent. only. The bottom velocity will be diminished, if at all, by a much smaller percentage, since it has been shown that the ratio of bottom to surface velocity increases rapidly as the depth decreases.

The above, which is merely an illustration, will suffice to indicate the course which it seems desirable to adopt in advising on the question, whether a given diminution should be allowed to be made in the tidal area of any particular river. The low-water discharge at some point below the proposed embankment should be carefully measured, and the total discharge of the inland or fresh waters should be measured at their entrances into the tidal area. The difference between the two will show the extent to which the low-water flow is increased by tidal waters.

The effect of the proposed operations may then be judged of by ascertaining how far they will reduce, not the cubic content of water passing up and down on each tide, but the area of mud, which is submerged by the tide, and thence contributes to the low-water flow. It should be noticed, however, whether this mud lies nearly horizontal, as inside a harbor, or inclined at a considerable angle, as on the banks of a river; since, in the former case, it will clearly retain the water longer, and therefore be a more effectual feeder, than in the latter.

This process is neither difficult nor costly; and the author submits that it would, at least, give a more satisfactory result than the course, at present common, of condemning embankments altogether. It would seem that embankments are only to be feared where the fresh-water flow is exceedingly small, or its current exceedingly sluggish. Many rivers have been embanked, especially in

early times, with favorable, rather than unfavorable, results. That the results should be favorable may, at first sight, seem impossible, but the author's experiments suggest a ready explanation. It will be remembered that the bottom velocity of the Avon remained at zero until the surface had sunk by about three-quarters of its total fall. This sinking is, of course, due to the passing away of the waters sent up by the flood tide. Now, if the Avon were embanked say, by building quay walls on each side from low-water mark, the content of these tidal waters would be reduced, but the slope of the surface and the velocity of the ebb would remain as before. Hence, the level would fall more rapidly, and the point at which the water at the bottom began to move would be at an earlier period of the ebb. The result of this would be to diminish the time during which sediment was being deposited, and lengthen the time during which it was being scoured away; and, therefore, it would be favorable, not unfavorable, to the maintenance of the channel.

Among recent instances of such embanking may be mentioned those at Aberdeen, at Lynn, and on the the Forth; and among early cases of the kind, it is impossible to forget that the three largest rivers of England, the Thames, the Severn, and the Trent, were all embanked in the course of the middle ages, with results the reverse of disastrous. Had the modern school of engineering existed in those times, it is at least probable that all those embankments would have been condemned and forbidden, and, in consequence, England would have failed to acquire many hundreds of square miles of her most fertile lands.

There is another operation which may be applied to tidal rivers, besides that of embanking their shores, *i. e.*, the process of "dockizing," or of excluding the tide altogether by a dam at or near the mouth, so as to turn the river, above this point, into a floating harbor or canal. Any individual case of this kind will have many peculiar circumstances, which must be carefully considered before a decision is arrived at, and no attempt will be made here to discuss the general question. But if there is any truth in the foregoing investigation, it at least shows the groundlessness of an objection frequently made to such proposals, namely, the fear of some unknown injury to the river thus treated, or to the estuary into which it flows. It appears that wherever silting takes place in a channel it is due to the tidal waters laying down a greater amount of deposit than they can sweep away; and that from this fact all tidal rivers, at least of ordinary size, would gradually silt up, if it were not for the corrective influence of the fresh-water flow. Thus, looked at from the point of view of maintaining the channel, the entrance of tidal water into an ordinary river can be nothing but an evil; whilst any deposit that may result in the estuary outside can be easily removed by judicious scouring at low water, to the effect of which the author can bear witness from his own experience. It would seem, therefore, that this mode of turning dangerous rivers into canals deserves fuller consideration than of late years it has received. It is well known that Smeaton recommended the "dockizing" of the Clyde, and it has since been remarked how happy the Corporation of Glasgow may esteem themselves in that they rejected his advice. The author ventures to think otherwise, and the labor of preparing this paper will have been sufficiently recompensed if it help to vindicate the wisdom of the first, and perhaps the greatest, of English marine engineers.

At a meeting of the Meteorological Society in June, a paper was read on "The Mean Direction of Cirrus Clouds over Europe," by Dr. H. H. Hildebrandsson, Hon. Mem. R. Met. Soc. The author has collected a number of observations on the movements of cirrus clouds over various parts of Europe, and after discussing them, has arrived at the following results:—(1) The mean direction at all stations lies between south-west and north west; (2) in winter the cirri come from a more northerly direction, and in summer from a more southerly; in winter the northerly component is greater on the Baltic and the north coast of the Mediterranean; (4) the mean directions of the upper currents nearly coincide with the mean tracks of storm centers; (5) the upper currents of the atmosphere tend in general to flow away from those areas in which a barometrical depression exists at the earth's surface towards those in which there is an elevation of pressure.

A SKETCH OF ROMAN BUILDING CONSTRUCTION.*

By W. T. OLDRIEVE.

From "The Building News."

To disentangle the architectural conceptions of a people from that which is borrowed or inherited by tradition, leaving that which can justly be called their own, is a task by no means easy. Architectural writers have, in attempting it, entered upon an arena of strife, which to the amateur and the professional student, is not a little perplexing. It is only necessary in this "sketch" to assume that we may legitimately call that Roman which the Romans first made a principle of construction or design, although the primary forms adopted may have been previously used by other nations. It is proposed to consider the subject under—I. Principles and forms of Roman construction. II. Processes and methods. III. Materials and workmanship.

I.—PRINCIPLES AND FORMS.

Although the Romans used the three "orders," it was in some important respects in a different manner to that in which the Greeks made use of them. The Romans did not rest satisfied with them, nor did they enter wholly into the spirit of an epistyle construction as the Greeks, whose purity of form and delicacy of line were never appeciated by the Roman architects. While the architecture of the Greeks is expressive of an artistic and refined simplicity, that of the Romans is expressive of vast resources, indomitable energy, and great constructive skill. The Greeks so designed their buildings that no decoration or adornment was needed as a necessity to make them artistic, for the artistic spirit is in the construction. Not so is it, as a principle, with the architecture of the Romans; the form which covers the construction is often independent of it. This may be stated as a first principle in Roman construction. Undoubtedly the most characteristic feature in the buildings of the Roman period is the arch. The Romans first used it in a true sense architecturally and consistently, although its use can be traced back, in a more or less perfect form, to almost all the ancient nations. In adopting the arch as an architectural feature, the column and the entablature were not discarded by the Romans, though they show by their great vaults and domes that they could constructively have been dispensed with. It is generally assumed that in the combination of arch and epistyle in the Roman façade, the arch and wall are Roman, and the column, &c., are borrowed from the Greeks. Semper has entered a protest against this assumption, and asks why the column should be borrowed by the Romans from the Greeks when both nations belonged to the same family and started with the same traditions, and when the column is found as early in Italy as in Greece. It is probable that the column and entablature were used as the natural construction, and the wall and arch introduced in the buildings of several orders in height, as the Colosseum, to support the upper stories as well as to form an outer screen. A feature was thus added which gave increase of strength, while preserving the columnar principle. Viewed in this way the Roman combination of column and arch is not an architectural scandal, though this is frequently taught. It is considered not improbable that this combination was introduced from some of the Hellenistic cities, though its introduction does not seem to have been clearly traced. The first Roman example is believed to be the Tabularium at Rome, B. C., 78. Another combination of the same nature, though less defendable, is the use of piers or mullions in an arch-headed aperture. This can only be mentioned here. The Pantheon at Rome exhibits, in its front external elevation, the curious effect produced by the pedimented portico placed in front of a domed circular structure. Fergusson and others have held that the portico is a later addition, but authorities are pretty generally agreed that this is not so. It is in the use of the arch principle as a vault that the Romans have gained pre-eminence. Vaulting was no

* Being the Cousin Prize Essay in the Class of Fine Art, Edinburgh University, Session 1884-85.

doubt used earlier in underground tunnels, &c., and the Hellenistic city of Mitylene in Asia Minor is supposed to have furnished the model for the first Roman stone-constructed theater on flat ground, B. C. 50, where arches supported the rows of seats; while a writer of about B. C. 40, referring to the city of Alexandria, says that the buildings were without boarding or timber of any kind, so that vaulting seems to have been in common use there. In no part of the architecture of the Romans can we get a better knowledge of the principles which guided them in their treatment of the arch. Although the vast extent of their undertakings seems at first sight to indicate that they were lavish in their expenditure of labor and materials, a more careful examination shows that there was really a feeling of economy, and that there is a correspondence between the extent of the building and the means adopted. By their scientific and constructive skill in the use of the vault, the Romans produced in their structures perfect solidity and incomparable grandeur with a minimum of expenditure. In following out our next division of the subject under consideration, we shall be able to trace in detail the carefully considered economy of parts and labor which was practiced in Roman buildings, as, for instance, the brick rib and concrete filling principle of vaulting, and the hidden buttresses in the thickness of the walls of the Pantheon, whereby greater resistance to the pressure of the vaulting was obtained than if the walls, 20 ft. thick, had been solid, though only half the quantity of materials was used.

II.—PROCESS AND METHODS.

It will be necessary, in order to keep within the limit of a short class-essay, to confine our inquiry to Roman building construction in its most limited sense, not giving much attention to the great engineering works of the Romans in the shape of viaducts, bridges, &c., which they have left in so many parts of the world. Again, of the processes and methods of build construction adopted by the Roman, only those which are peculiarly characteristic need be referred to. The arch and vault were constructed both of stone and of brick and concrete; the round arch, the barrel and the domed vault being almost the only forms used. When of stonework, the arch stones, or "voussoirs," were carefully hewn to the proper form and built in alternate bands, thus economizing the centering. The old Roman baths at Nimes (now called the Temple of Diana) offer a good example of this construction. Another form of stone barrel vaulting was constructed with stone ribs and slabs between, and is interesting as bearing a resemblance to what is found in some early Christian churches in Syria. It seems a little astonishing that the Romans with all their constructive skill, avoided groining in their stone cross-vaults. Wherever square stone-vaulting met, the stone construction would be stopped and the vault worked in brick and concrete. In sloping vaults, as the entrance passages to the amphitheaters, the arching was sometimes executed in stepped rings, each separate arch standing vertically, and thus exerting no pressure upon the lower part of the vault. The inside of the stones would, in this construction be dressed off to the slope, probably after the stones were set in position. In the passages with converging sides the voussoirs were sometimes arranged thus, with the line of keystone and the lines of springing stones of parallel widths, the filling-in stone being wedge-shaped on plan. At other times the side and key stones were wedge-shaped on plan and the filling-in stones were parallel. In the construction of the oblique arch, the abutments of the stone rings were built at right angles to the faces of the rings, the stones being cut to the required obliquity. The brick and concrete vault is most characteristic of Roman construction, although it does not date further back than the time of Augustus. It is thought probable that this too came from the Hellenistic cities. Had the great Roman vaults been "cast" entirely of concrete, the timber centering would have shrunk during the execution of so extensive a work and caused cracks in the concrete which might have led to an entire collapse. Viollet-le-Duc calculated that the centering for the dome over the Pantheon would have sunk about 20 inches in from three to six months if constructed over the whole area at once, like an inverted basin. To avoid this, and to give elasticity to the whole covering, the Romans constructed light ribs of brick and filled in between them with concrete. The

brick ribs, unlike those in the Gothic periods, were not intended to be decorative features and so were not permanently exposed, The concrete filling was economical, only easy straightforward labor being necessary. I may also mention a building on the Palatine at Rome. Here the ribs are made up of cells formed by large flat bricks, 2 ft. square, and smaller bricks 2 ft. by 6 in., their thickness being 2 in. The cellular construction was sometimes applied to the whole vault, as at the Palatine, and as here illustrated. The cells gave support to the concrete, both by adhesion and by their wedge shape. The concrete filling between the brick ribs appears to have been sometimes arched on the upper surface; but it is not very easy to see how this could have added much to the strength of the vault. The great example of Roman barrel vaulting is that over the Basilica of Constantine, near the Colosseum at Rome, the span being no less than 80 ft. The casettes here, as in the Pantheon, and other examples, greatly reduce the weight of concrete, while not causing any decrease of strength, for where the casette is most deeply sunk, an approximation to the form of an arch is formed, as shown by dotted lines on sketch. The wood moulds fastened to the centering before the concrete was filled in would greatly assist in securing the ribs in their position laterally. Exception might perhaps be taken to the smaller sinkings in the face of the ribs, as in the Pantheon. If the ribs were intended to be displayed, these sinkings would certainly be most objectionable, but the objection is at least answerable when we recognize that the principle of the Roman architects was not to display the parts of construction, but to construct a framework and apply a covering, thus following the principle of overlaying, which is observed to govern many styles of architecture. There is still another method of constructing the vault which was used by the Romans, namely—that by which the vault was formed by layers of tiles or flat bricks over the centering. The arching was reduced, as much as possible, by carrying up the wall considerably above the line of springing. This system has the advantage that skeleton centering would serve the purpose of supporting the tiles at their joints. This construction is still in use in Italy. In dealing with cross vaults the Romans preferred to have two barrel vaults of the same span, but they sometimes stilted one vault to have both crowns on a level. Oblong vaults were occasionally used, but the square were preferred. The groins in cross vaulting of the brick and concrete construction were formed in a similar manner to the ribs already described; the lower edges of the bricks, however, had to be either specially molded to the proper angle before the bricks were burnt, or cut to the exact line after being set in position. Where the groin ribs meet at the crown of vault no attempt was made to make a finished junction or miter, one rib was carried over unbroken, and the other simply butted up against it on either side. A sketch of the cross vaulting over the Therme of Diocletian will illustrate what has been said. Domical vaulting was executed by the Romans in a very similar manner to the brick rib and concrete barrel vaulting. At the Pantheon the ribs are probably of brick, and carried up of the same width to the top, where they abut against the curb of the hypæthral opening. Viollet-le-Duc thus vividly describes this master-piece of spherical vaulting:—"What, in the Pantheon at Rome, is it that produces the most lively impression? It is that immense vault which derives all its decoration from its very structure; it is that single opening for light, 26 ft. in diameter, perforated in its summit, through which the zenith is seen, and which throws upon the pavement of porphyry and granite a large circle of light. It is there that the genius of the Roman appears in full strength. So great is the elevation of this orifice above the floor, that its enormous opening scarcely affects the internal temperature. The most violent storms scarcely send down a breath of air on the head of a person standing beneath its orbit; and when it rains, the drops are seen falling perpendicularly down upon the pavement of the Rotunda, on which they describe a circle of wet. The cylinder of rain drops, falling from that height through the space of the building, renders sensible the immensity of that space. It is in conceptions like this that the Roman is really grand, because they are the outcome of his own genius, and because for their execution he borrows from no one, nor asks

the aid of any artist whose nature is foreign to his." The diameter of this building is 143 ft., the height from floor to top of dome is the same, and the springing of the vault is half way up. The spaces between the ribs are filled in with concrete and casseted or coffered, with four sinkings so arranged that a spectator in the center of floor sees all the margins of sinkings of equal width. A rosette of gilded bronze was originally placed in the center of each coffer, and bronze ornaments decorated the borders. An old writer states that he saw the manner of the construction of the dome during some repairs, and that there are series of arches. If this is the case, this outer system of arched reticulation must be regarded as an additional means of bracing the cupola. Nothing need be said of the portico, except that its roof was originally supported by bronze girders of the section of an inverted rectangular trough, which fact is interesting as pointing to the existence of so early an example of tubular metal girders. Prof. Adler has described the scientific construction of this building, and points out that the cupola is really supported by eight pillars and strengthened by the upper buttresses, the remainder of wall being merely filling in between the constructive parts. Prof. Adler's restoration of the upper story of the Pantheon is now generally acknowledged by authorities. Roman domes and vaults were not protected by outer roofs as in more modern examples, the interior only being considered by the architects.

III.—MATERIALS AND WORKMANSHIP.

The ordinary mason's work of the Romans was not executed in large blocks of stone, as the smaller were more economically worked. It is true that large blocks of stone are found, of Roman work, in the temple at Jerusalem ("Opus Quadratum"), but they are evidently in imitation of the older work of Solomon's temple. Foundations and first courses are often of stone when the other parts of walls are of brick strengthened by timber. Two kinds of exterior facing to stone walls are mentioned by Vitruvius as commonly used by the Romans, "Reticulatum" (net-like), and "Incertum," (irregular), Vitruvius says that the former was the method in general use in his day, and the latter the ancient method. A limestone called Travertine was much used in Rome; the Colosseum and other buildings being built of it. Travertine, Saxum Tiburtinum, is a calcareous deposit found along the course of the Anio and Aqua Albula, by whose waters it is deposited, but most plentifully in the neighborhood of Tivoli, the ancient Tibur, whence its name is derived. When first taken from the rock it is soft and easily cut, but the longer it is exposed to the air the harder it becomes. This may be ascertained by comparing its state in the native rocks near Tivoli with the exterior walls of the Colosseum. For less exposed work, as the interior of walls, and where covered by plaster, soft Tufa was used, and a hard volcanic substance called Pepperine, which could be exposed to the air with more freedom. Very many buildings in Rome are composed of it. Basalt was principally used in paving roads and streets; but it is also to be found employed in consolidating and packing other stones, as in the interior of the wall of Servius Tullius, &c. Pumice was only used to lighten a building, as in the vault of the Pantheon and in the Colosseum. Marble was imported for monolithic columns, and was much used in slabs for lining interior of walls, &c. It was the boast of Augustus that he had found Rome brick and left it marble. The chief supply of white marble was derived from Luna, called "Marmor Lunense," and the same quarries are still worked, the marble being well known by its modern name of Carrara. The white marbles of Greece were also occasionally employed. Of colored marbles the most esteemed are the Rosso, Verde, and Giallo Antico, and Cipollino, Africano, and Pavonazetto. Roman bricks were of different forms and sizes; flat tile-like bricks were generally used, some found 2 ft. square and 2 in. thick, others $1\frac{1}{2}$ ft. by 1 ft. and $1\frac{1}{2}$ in. thick. A triangular form for "opus latinitium," was also used in facing. Vitruvius devotes a chapter to a description of bricks. About the time of Constantine a practice was introduced of using large jars of baked clay (Testæ) to diminish the weight of a dome or the upper part of a wall. The Romans are famous for their mortar and cement, the excellence being due to the use of Puzzolana earth, which was used along with the lime. The Puzzolana is a volcanic substance and very

abundant in every part of the Campagna di Roma, and the extent to which it was used is shown by the vast caverns which form the catacombs. The name Puzzolana is said to be derived from the Pulvis Puteolana, which was a similar substance found near Puteoli, now Puzzuoli. Concrete was most used, as we have already seen in considering the processes and methods of construction. It was always covered with a facing of finer material. The method of laying concrete was by an even bed of mortar 6 in. to 8 in. thick, first put down, and then the stones pressed into it while soft, until the desired solidity was attained. This process was, of course, only applicable where there was resistance to lateral pressure. Plaster work (intonacum) was brought to great perfection, particularly for coating the specus of aqueducts, the interior of piscinæ, and those walls of houses which were adorned by fresco painting. Metals were not much used. The bronze beams of the portico of the Pantheon have already been alluded to. The doors of this building were of the same material. Iron was used for clamps in stonework. Lead pipes for water service are supposed to have been used. Glass was sparingly used for lighting, and as early as the time of Agrippa, was employed in mosaic work. Mosaic pavement was introduced into Rome in the time of Sylla and was largely used. Tiles appear to have been the only covering made and used for roofs. Roman works were carried out by either, 1, paid labor; 2, forced labor; or, 3, soldier labor. The materials were obtained either, 1, bought; 2, taxed; 3, procured from the quarries, &c., by forced labor. The regular workman had considerable political power. Trade guilds were formed and recognized by most Emperors, after it was found impossible to put them down. Immunities and privileges were granted on certain conditions, as that Government works should be carried out at fixed rates, &c. Time will not now permit of further notice of workmanship, and so this paper must close with a word of admiration for the constructive and organizing genius of Roman architects.

EXPERIENCE IN THE USE OF THICK STEEL BOILER PLATES.*

By W. PARKER.

From "Iron."

An ordinary cylindrical boiler of 13 feet diameter and 16 feet long, designed for a pressure of 150 lbs. per square inch, for which the scantlings were amply sufficient, burst under the hydraulic test. The pressure was applied very carefully, and when it had reached 240 lbs. the fracture occurred, extending completely across one of the shell plates, and to a slight extent, also, into the adjoining plate, as shown in diagram. The boiler was constructed entirely of steel made by the Siemens-Martin process, by a firm who enjoy the reputation of producing a material second to none in the country. The plates were all tested at the steel works and fulfilled the requirements of both Lloyd's Register and the Board of Trade. I find from our surveyor's report that the sample from the particular plate which failed—which was 1¼ inch thick—stood a tensile strain of 29.6 tons per square inch with an elongation of 20 per cent. in a length of 8 inches, whilst strips cut from it were bent almost double, cold. In fact the material appeared, from the mechanical tests applied before it left the steel works, to be in every respect suitable for the purpose for which it was intended. One remark, however, may here be made, namely, that the plate in question was exceptionally large and heavy—viz., 20 feet long, 5 feet 6 inches wide, and 1¼ inch thick, weighing about 2 tons 16 cwt.

This material was built up into a boiler by a company who have had an unusually extensive experience in the manipulation of steel, having turned out no fewer than 175 boilers of this material. The plates were treated precisely as other steel plates have been treated in the same works, and

* Paper read before the Institution of Naval Architects.

with all the appliances which experience has shown to be necessary, all the holes were drilled, and the plate was then heated in a furnace and bent to the required curvature in a pair of powerful vertical rolls in the usual manner. Under these circumstances it appeared at first sight astounding to find the material tearing under a pressure which represents a strain of 6.7 tons per square inch only, or less than one-fourth of the strain which the original sample withstood. In addition to this the appearance of the fracture indicates that the plate did not possess any ductility, stretch, or elongation whatever. Neither the steel makers nor the boiler maker have as yet afforded any satisfactory explanation of the occurrence. It is without doubt a most serious affair, especially in view of the high pressures which have now become so common. On hearing of this accident the committee of Lloyd's Register instructed me to investigate the matter, endeavor to ascertain the cause of the accident, and, if possible, recommend some measure to prevent such an occurrence in the future. My investigations were only completed last Tuesday, and as such a serious matter as this, which bears upon the probable safety of life and property at sea, must naturally give rise to no little speculation amongst engineers and steel makers, and has already produced great consternation in many quarters, I have taken this opportunity of laying before you a short statement of the facts as they have come before me, the results of my investigations, and the conclusion which I have arrived at, with a view to eliciting from the various steel makers and steel users here, the benefit of their views and experience.

Upon my visit to the boiler-making works I was fortunate enough to find a sister boiler to the one which had burst, ready for testing. This boiler was tested in my presence to 300 lbs. per square inch, and was carefully measured and ganged, and found to show no signs of deflection or yielding. I also ascertained from an examination of the testing appliances that an abnormal pressure could not possibly have been exerted at the time of the testing of the first boiler. Seeing that the plates that broke had stood all the mechanical tests required before leaving the steel works, and that when worked into the form of a boiler shell it gave way at less than one-fourth of its original strength, it appeared at first sight that the plates had been in some way injured, or had undergone some material change from the time they left the steel works until they were riveted into the form of a boiler shell; therefore it became necessary to look carefully into the mode of manipulation of the plates in the boiler-shop, and especially the heating and bending of them. One of the plates was bent in my presence. It was heated in an ordinary plate furnace, but when taken out was far from being of a uniform heat; the end of the plate near the door of the furnace was at a black heat, which gradually increased toward the other end to a dark red heat. Then the plate was turned end for end and again placed in the furnace with a view to heating it, as far as possible, uniformly; but when again drawn out of the furnace it was seen that the heat was not at all uniform, one end being of a dark red or nearly black heat, which gradually cooled down to a blue heat at the other end.

In this condition it was passed through a set of powerful vertical rolls, and bent to the required curvature. The plate passed through these rolls six times, and by the time the operation was completed, one end of the plate was quite cold, while the other end remained at a blue heat. It was thought that this unequal heating of the plate may have set up in the body of the plate excessive strains of a dangerous character, and that these strains were aggravated by rolling the plate at a dangerous heat, it being well known that the ductility of all steel becomes lessened when worked at a blue heat, and it is, I think, generally admitted that it is far safer to work steel cold, or red hot, than at any heat between these two points. Steel plates, and especially large ones, must be injured by such treatment, but as to the intensity of the strains set up, or their exact locality, nothing definite can be said. To ascertain the nature of the material as it stood, test pieces were cut from the fractured plate, both close to the fracture and apart from it, and subjected to tensile test at one of Lloyd's proving houses, with the following results, which the engineers have kindly communicated to me:

Samples.	Breadth.	Thickness.	Area.	Total Tensile Strain.	Strain per Sq. In.	Extension In 8 Ins.	Extension.	Contracted Area.
	Inch.	Inch.	Sq. In.	Tons.	Tons.	Per cent.	Inches.	Inch.
S. I. X.....	1	$1\frac{13}{50}$	1.26	40.5	32.14	27.34	$2\frac{3}{16}$	$\frac{11}{16}\times\frac{7}{8}$ & $\frac{1}{32}$
S. C. H. I....	1	$1\frac{13}{50}$	1.26	41.75	33.1	26.59	$2\frac{1}{8}$	$\frac{29}{32}\times\frac{29}{32}$
S. 2.....	1	$1\frac{13}{50}$	1.26	41.5	32.93	21.27	$1\frac{11}{16}$	$1\frac{3}{32}\times\frac{15}{16}$
S. C. H. 2 X.	1	$1\frac{13}{50}$	1.26	39.5	31.35	23.4	$1\frac{7}{8}$	$1\times\frac{3}{4}$
S. XX.....	1	$1\frac{13}{50}$	1.26	37.5	29.7	21.8	$1\frac{3}{4}$	$\frac{13}{16}\times1$
S. IXX.....	1	$1\frac{13}{50}$	1.26	37.25	29.56	26.6	$2\frac{1}{4}$	$\frac{31}{32}\times\frac{25}{32}$
S. XXX.....	1	$1\frac{13}{50}$	1.26	38.5	30.5	28 1	$2\frac{1}{4}$	$\frac{11}{16}$ & $\frac{1}{32}\times\frac{15}{16}$
S. IXXX....	1	$1\frac{13}{50}$	1.26	38.25	30.3	27.34	$2\frac{3}{16}$	$\frac{29}{32}\times\frac{3}{4}$

From these tests it appears that the proved tenacity of the plate ranges from 29.5 tons to 33.1 tons, while the elongation ranges from 21.8 per cent. to 28.1 per cent. in a length of 8 inches. I may say that I corroborated these tests by others made from the same plate for my own information in London, and they were also corroborated by other tests made for the information of the steel makers. This range of about 4 tons in the tensile strength of a plate of homogeneous metal like mild steel is very unsatisfactory. I obtained samples of the plate, and submitted them to five eminent and independent metallurgists, who have kindly furnished me with the results of their chemical analyses, which are as follows:

Carbon.	Silicon.	Sulphur.	Phosphorus.	Manganese
0.36	0.015	0.055	0.087	1.05
0.27	0.016	0.044	0.076	0.641
0.33	0.010	0.038	0.065	0.612
0.30	0.018	0.044	0.063	0.648
0.26	0.005	0.038	0.067	0.650

The most striking feature in these analyses is the large proportion of carbon shown to exist in the plate. It is particularly high for boiler plates. Material used for thin plates, say, from $\frac{1}{2}$ inch to $\frac{3}{8}$ inch thick, to stand the same mechanical tests as these thick plates did, would not contain more than from 0.15 to 0.18 of carbon; and these facts led us to further experiments. In view of the great difference in the amount of carbon required in steel for a thick plate and a thin one to stand the same mechanical tests, it was deemed desirable to make an experiment which would determine to what extent work in the shape of rolling—and especially rolling thin plates, which, during the latter part of the operation, must of necessity be rolled, comparatively speaking, cold—affected the tenacity and ductility of the material. A slab of steel containing about the same amount of carbon as the plate that ruptured, viz., 33, was obtained at the steel works where the plate was made, and rolled at one heat down to $\frac{1}{2}$ inch in thickness. This material, had it been rolled down to $1\frac{1}{4}$ inch plate, judging from the carbon it contained and the tests of the broken plate, as well as the opinion of the steel makers, would have had a tenacity of from 30 to 34 tons per square inch. It was found, however, that when rolled down to $\frac{1}{2}$ inch thick, its tenacity was increased to from 35 to 41 tons per square inch, with an elongation of from 21 to 24 per cent. in a length of 8 inches. Other pieces were made hot and quenched in water. These, when tested, broke at a tenacity of from 44 to 45 tons, and had, practically speaking, no stretch at all.

Pieces were cut from the fractured edge of the plate, and subjected to tensile, bending, and temper tests. They showed a tenacity of 33.5 to 34.2 tons per square inch, but they stretched only 13 and 16 per cent., and broke with a crystalline fracture, as will be seen by the specimens produced. They bent cold to a considerable degree, but when made red hot and quenched in water, instead of bending, as pieces of a thin plate of similar tenacity and ductility would do, they broke under the first blow of a hammer without any bending whatever. The material was so high in carbon as to take a temper and become quite hard and brittle. Further cold bending tests were made from pieces of the broken plate, both before and after being annealed; those which were tested before annealing bent fairly well, strips $1\frac{1}{4}$ inch square bent to an angle of 49 and 61 degrees, the fracture showing a considerable amount of alteration in form; while those pieces which were tested after annealing, bent much better, in fact, al-

most double. Strips, however, that were heated and quenched in water, broke short without any bend whatever at the first blow of a hammer, and thus corroborated the previous experiments made in London. These experiments point to the fact that the plate which gave way must have become partially tempered by the heating and cooling to which it was subjected for the purpose of rolling it into its cylindrical form. The heating not having been uniform, the tempering could not have been uniform, and the variations in the temper, no doubt, have caused the variations in the strength and ductility shown by the different parts of the plate. The hardest part of the plate yielding less than the rest became naturally more strained, and hence the plate tore at its hardest part at a pressure only a small fraction of that which it would have borne if its yielding had been uniform.

Having thus placed before you the nature of this accident, and the steps taken with the view of unraveling the supposed mystery, I now venture to state what inferences may, in my opinion, be drawn from the results of the investigation. I think it will be acknowledged that a material which is so high in carbon as to take a temper and break short as described, even if it possesses high qualities of tenacity and ductility before being tempered, must be looked upon as unreliable and altogether unsuitable for use in marine boilers. It would appear that the desire to obtain high steam pressures, and to use steel of a higher tenacity consistent with a large amount of ductility, has caused the marine engineering world to unknowingly drift into using a material of an unreliable and unsuitable character for the shells of marine boilers, more especially when the usage which such plates receive in heating and bending is considered, for, except among steel makers, it does not appear to have been generally known, that the thicker a plate is, the more brittle and erratic in its behavior it must become, as compared with a thin plate made to stand the same mechanical tests as far as tenacity and ductility are concerned, as, otherwise, I feel convinced that the increase in tenacity from 29 to 32 tons for thick boiler shells would not have been advocated.

So far as I am concerned, and the society which I represent, I may say that it has always been our endeavor to discourage the use of steel of high strength. The rules of Lloyd's Register require boiler-plates to have a tensile strength of from 26 to 30 tons, and have done this from the commencement of the use of steel, because we felt that the higher the tenacity arrived at, the more likelihood there would be of the plates giving trouble, and our whole desire has been to keep the material mild. We have, however, had considerable pressure brought upon us by manufacturers and engineers to allow a strength of 32 tons per square inch for thick boiler shell plates. This accident and the investigations which have followed, clearly point out that engineers have been drifting toward the use of an unreliable material, or at all events a material which is too near the verge of danger to be pleasant, a state of things that should not exist with steam boilers.

I would therefore urge, in order to remedy this growing evil, that the tenacity of steel plates for boiler shells—which are becoming thicker every day, should in no case exceed 30 tons; and that a temper test should be insisted on from every thick plate, and the practice of using enormously large plates should be discouraged, while more care should be exercised in uniformly heating and bending these plates. I have conferred with the principal steel makers in the kingdom on this subject, and am able to say that they agree with me, and are decidedly of opinion that steel plates over an inch in thickness, and having a tenacity of more than 30 tons, must contain so much carbon as to render them unsuitable for boiler-making purposes, although they may possess the necessary tenacity and ductility to withstand the usual tensile and cold-bending tests. I venture to hope that this paper will be made the subject of discussion, with a view to obtaining further opinions respecting the important points in question.

Nature says it is contemplated to use the electric light in Algiers for night work during harvest time, in order to escape the heat, which is just too much for Europeans, and is an obstacle to their carrying on agricultural work.

ON THE ANTISEPTIC TREATMENT OF TIMBER.

By SAMUEL BAGSTER BOULTON, Assoc. Inst. C. E.

From Papers of the Institution of Civil Engineers.

III.

Mr. Boulton had been obliged to be very brief in his verbal reply at the close of the discussion, and as some of the points then raised involved matters of considerable detail, which had also been alluded to in the correspondence, he thought that unnecessary repetition would be avoided if he were to connect his replies to both series of communications in a continuous form. He was gratified at the valuable support which his main propositions had received.

The remarks made by Dr. Tidy, and the views expressed by that gentleman in his recent report to the Gaslight and Coke Co. were in principle in accordance with the views expressed in the paper. The author, however, believed with Dr. Armstrong that Dr. Tidy, who had been somewhat conservative on the subject of tar acids, would be led by the logic of facts to accept a much lower proportion than 8 per cent. The "London creosotes" as they came from the still, honest creosotes which had done excellent work, and which constituted probably about one half of the total supply of this kingdom, did not contain so large a percentage. Some misapprehension still existed on this subject, which the statement of a few facts might remove. In July, 1863, the author sent to Dr. Letheby a sample of the usual London creosotes, which he was then largely using. Dr. Letheby found it contained only 4.37 per cent. of tar acids. Later on, and during one period of seven years especially, nearly the whole of the tar of the great London Gas Companies, as well as tar from other sources, was contracted for and distilled by the author's firm. The quantity was probably larger than had ever been treated up to that time by any one firm or corporation, and it therefore formed a sufficiently broad basis for estimation. He would give the quantities during three consecutive years—

1877	Gallons of tar distilled....	14,735,404
1878	" " "	15,839,819
1879	" " "	12,690,029

or an average of between fourteen and fourteen and a half million of gallons per annum. He had found, as stated in the paper, that the heavy oils distilled from this mass of tar contained on an average from 4 to 7 per cent. of total tar acids. More recent experiments which he had made upon a large number of London tars—one series in May, 1882, another in August, 1882, and a third since this paper had been read—gave similar results. Latterly, the largest of the English gas companies, the Gaslight and Coke Co., had erected works at Beckton, at which they distilled their own tar. It had been assumed that the list of analyses appended to Dr. Tidy's printed report represented the percentage of tar acids which the London creosotes in their natural condition contained. This, however, was not the case. The samples analyzed by Dr. Tidy contained from 8.2 to 10.2 per cent. of tar acids, but they had been specially treated to "meet the market," created by the modern type of specification by removing from the creosote some of its least volatile parts, those parts containing little or none of the volatile tar acids. The Gaslight Co.'s creosotes as they came from the still contained on an average 6 per cent. of total tar acids by the ordinary caustic alkali test. The author had been enabled to clear up this matter, of which experts would readily detect the importance, owing to the courtesy of the Board and Secretary of the Gaslight and Coke Co.

He agreed with Dr. Armstrong in the importance of M. Pasteur's experiment upon sawdust, which was recorded in the Comptes Rendus of the Académie des Sciences for 1863. It is remarkable as an early demonstration of the application of the germ theory to the phenomena accompanying the decay of woody fiber. Dr. Armstrong had alluded to the distinction between wood creosote and tar creosote. Both contained tar acids, some of which might be identical, or if not identical, isomeric. But tar creosote, if

it could be so called, was a complex body; some of the tar acids it contained differed essentially from either carbolic or cresylic acid, being less volatile, and less soluble in water than either phenol or cresol. There is evidently room for much further investigation in this connection; also for a more complete comparison between the "tar acids of the coal-tar oils and similar bodies contained in other oils."

In relation to the remarks of Mr. W. Foster, the author must express the hope that that gentlemen would continue the very interesting researches of which he had so recently given an account to the Institution in his valuable paper on "The Composition of Coal." Authentic Tables as to the varying products derived from different kinds of coal, and at different temperatures, were becoming matters of the first necessity in various branches of industry. Mr. Foster had referred to the experiment of Pettigrew, alluded to in the paper. Pettigrew had removed the embalming material from the heart of the mummy by steeping it in alcohol; after which, upon exposure to the atmosphere, putrefaction took place. What the author desired to point out was that the previous immunity from decay had not been the result of any chemical combination between the antiseptic and the tissue.

A jarring note had been struck by Mr. Bamber, who had represented "the whole secret of the paper" to consist in "the author's idea.....that nothing should be left in the creosote which it would pay him better to take out;" an object foreign to the declared aim and intention of the paper. The author had not approached the subject from the commercial point of view—a fact which the President had so gracefully recognized. It might, however, be opportune to state that he was not at present commercially interested in any manufacture which caused carbolic acid to be "taken out" of the creosote oils, although he was largely interested in the success of prepared timber as an engineering material, and therefore in the choice of the best antiseptics for that purpose, whether obtained from the creosote oils or from other sources. Mr. Bamber's figures as to the comparative commercial values of creosote oils and carbolic acid, recalled to memory the well-known comparison between the value per ton of iron ore and of steel watch springs. The manufacture of pure carbolic acid was a long and costly process, of which the first cost of the crude material formed an altogether insignificant item. Nor was so low a price as 2*d.* per gallon for creosote either "proverbial" or usual. But it would be found in the long run that the consumer had to pay the commercial value for everything which the creosote contained, and it was therefore best to discuss upon scientific and practical grounds the substances which the engineer should require it to contain. It was one of the main objects of the paper openly to point out by diagrams and detailed descriptions the principal substances contained in the coal-tar oils, to draw attention to their properties, and to state their uses for various manufactures, so that for the purposes of timber-preserving, engineers might be in a position to "prove all things, hold fast that which is good." Mr. Bamber was mistaken as to facts in his allusion to Dr. Letheby's specification, and that of Dr. Tidy. Dr. Letheby's specification, drawn up under instructions from Mr. Meadows Rendel, M. Inst. C.E., in 1865, for the use of the East Indian Railway Company, stipulated that the creosote was to yield to a solution of caustic potash, not less than 5 per cent. of crude carbolic, and other tar acids. Dr. Letheby never increased that quantity. Dr. Tidy had increased, and not as Mr. Bamber supposed, diminished the percentage of tar acids mentioned by Dr. Letheby.

Mr. Bamber complained that no facts or data had been given respecting Dr. Tidy's experiments on naphthaline. But the paper contained a reference to a printed report of Dr. Tidy, deposited in the library of the Institution, wherein was a full account of these experiments. They were also recorded and approved of by Dr. Lunge, of Zurich, in his learned work upon "The Distillation of Coal Tar." Amongst other authorities who after investigation differed from Mr. Bamber in admitting naphthaline as an ingredient in the timber-preserving oils, were the late Mr. Bethell, Mr. Burt, Prof. Sir Frederick Abel, Mr. Forestier, for the French Government, Mr. Coisne, for the Belgian Government, &c. Mr. Bamber had once stated to an eminent engineer, in a report upon a creosote highly charged with naphthaline, that timber impregnated with such

an oil would, "within a very short time of the timber being in India, lose 5 lbs. out of every 10 lbs. put into the timber here merely by escape of naphthaline." Dr. Tidy's experiments with timber injected wholly with naphthaline, and subjected to a temperature of 130° Fahrenheit, proved that these apprehensions were unfounded. But it was now related by Mr. Bamber that in his own experiment a piece of wood impregnated with a creosote of the type which he preferred, and containing 20 per cent. of tar acids, lost in four weeks 42.33 per cent. of the oil taken up. Mr. Bamber's record of his own experiment was very instructive. He tried two kinds of creosote against each other. One, which might be called specimen A, was "full of naphthaline," but the percentage of that body was not stated. It contained 10 per cent. of tar acids. Specific gravity not named. With this oil a piece of deal 3 inches by 3 inches by 8 inches was impregnated. The other, which might be called specimen B, was a "country oil," specific gravity 1.045, containing 20 per cent. of tar acids. With this oil a piece of deal 3 inches by 3 inches by 6 inches was impregnated. Specimen A was alluded to as "Mr. Boulton's own oil" and "the author's London creosote;" but to these appellations he demurred, as he never used a 10 per cent. creosote unless required to do so by specification, and the London oils did not in their natural state contain 10 per cent. of tar acids. Therefore A, although it might come from his works, would be a mixture of London and Country oils. But, although in the author's judgment too volatile, yet the 10 per cent. specimen would be less volatile than the 20 per cent. Therefore, the author preferred A to B. Where a large issue was staked upon a single minute experiment, accuracy of result should be ensured by the most minute precautions. It was not explained why the two pieces of deal were not cut to the same size, a circumstance which affected the conditions both of absorption and of evaporation. Nor were the specific gravities of the two pieces of wood stated. Of two pieces cut from the same log, one piece of wood would frequently absorb, under the same conditions, a very much larger quantity of fluid than the other. However, the results as stated might be calculated as follows:—

A. Piece of wood, capacity 72 cubic inches, absorbed 1,020 grains of creosote =3.49 lbs. per cubic foot.

B. Piece of wood, capacity 54 cubic inches, absorbed 1,785 grains of creosote =8.17 lbs. per cubic foot.

But no pressure was used, and engineers would recognize that the experiment failed to reproduce the conditions of the ordinary creosoting cylinder. It was well known that without pressure, light oils penetrated timber more easily than heavy oils. In like manner the adulterating substance, bone oil, penetrated more readily than creosote; solutions of metallic salts more readily still; and water more readily than all. But it was "light come, light go;" those which penetrated most readily were generally the least permanent. The main object of the engineer was not to select the fluid which gave the contractor the least trouble to inject. He desired to select the antiseptic which was likely to be the most efficacious and the most permanent, and he required the contractor to provide efficient apparatus, and to inject under pressure a stipulated quantity by weight. Sleepers and large logs of timber were injected without difficulty with creosotes of a heavier type than either of Mr. Bamber's samples, and to the extent of 10 lbs. and 12 lbs. per cubic foot. Small pieces of wood could be easily gorged with creosotes. The author had recently injected some fir paving blocks 6 inches by 6 inches by 3 inches, with 22 lbs. per cubic foot of ordinary heavy London creosote, containing about 5 per cent. of tar acids. Mr. Bamber exposed his specimens to evaporation on a mantelshelf at a temperature never above 70° Fahrenheit, and generally between 40° Fahrenheit and 50° Fahrenheit. In four months A had lost 47.75 per cent., and B had lost 42.33 per cent. of the creosote put in. If this could be taken as a normal result, engineers would hesitate as to employing either type of creosote. No doubt both were too volatile. But it should also be borne in mind that the injection was imperfect; to use Mr. Bamber's "expression, it was only "skin deep." As regarded the comparative evaporation of the two specimens, however, the result was extremely valuable. It is well-known that the evaporation of fluids (except when in a state of ebullition) was in pro-

portion to the surface exposed, and not to the bulk of the fluid. This point Mr. Bamber appeared to have forgotten; he had exposed A, the creosote he disliked, to a wider evaporating surface than that to which he had exposed B, the creosote which he preferred. The position on the mantelshelf in which the pieces of wood were placed was not stated. But supposing them to have been suspended, say by a thread, so that all the surfaces were exposed to evaporation equally, the results might thus be calculated:

A. Piece of wood, the sum of whose superfices was 114 square inches, lost 487 grains=4.29 grains per square inch of exposed surface.

B. Piece of wood, the sum of whose superfices was 90 square inches, lost 575 grains=6.39 grains per square inch of exposed surface.

If, however, each piece of wood had been placed with one of its sides in contact with the mantelshelf, so that one surface was protected from evaporation, the calculation became slightly modified, so that A would have lost 5.41 grains, and B 7.98 grains per square inch exposed. If the specimens had been placed on end, then A showed a loss of 4.64 grains and B of 7.09 grains per square inch. Mr. Bamber had therefore been mistaken as to the comparative volatilities of naphthaline and the tar acids, as proved by his own experiment. B, the creosote with 20 per cent. of tar acids, had lost about 50 per cent. more than A, the creosote with 10 per cent. of tar acids and "full of naphthaline." Had it been otherwise, every chemical treatise describing the properties of these bodies, would have to be re-written. The statement that part of the loss of specimen B was due to the fact that some of the oil drained out of it, which it was said "was not fair" to that specimen, gave rise to the rejoinder, was it quite fair to a timber-preserving process that a type of antiseptic should be recommended which "drained out" with so little provocation? This part of the discussion might almost appear trivial, were it not for the fact, confirmed by many special instances in the author's experience, that whenever these light oils had been used exclusively, whether for marine work or for railways, complaints invariably arrived, sooner or later. Oils of so light and volatile a nature lost a large portion of their bulk, which evaporated or drained out in the creosoting yard, on the export ship, and on the permanent way in India and elsewhere. An experiment, easy to carry out without any laboratory apparatus, may be tried by any one interested in this subject. Take three saucers or shallow dishes; place in one saucer 200 grains of pure carbolic acid (crystallized), in the second 200 grains of pure cresylic acid, and in the third 200 grains of pure naphthaline. Expose them side by side in any room, and at any ordinary temperature. The crystals of carbolic acid would liquefy in a few minutes, owing to the avidity with which that body absorbed moisture from the atmosphere. In a few weeks' time (varying with the temperature) the carbolic acid would have entirely disappeared by evaporation. By that time the cresylic acid would have lost about half its bulk. When the whole of the cresylic acid had also evaporated. The naphthaline in considerable bulk, at least one-half of the original weight would still remain, an easy victor in the trial of endurance.* The evaporation was greatly retarded by the incorporation of those bodies with the less volatile oils, and by their being driven into the cells of the timber. But the evaporation must necessarily take place in proportion to the respective and recognized volatilities.

Allusion had been made by Mr. Bamber to "charred oil," and he presumed that it was a residue of anthracene manufacture. The author in the course of his experience had never met with "charred creosote," except indeed as a result of over-heating in a laboratory experiment; nor was he acquainted with any ordinary process of manufacture by which it could be produced. Creosote oils were distillates; whatever the heat in the still, the residuum might become carbonized, but not the substances which came over in the form of vapor. Anthracene or paranaphthaline had been denounced by the creosote specifications of the theorists at a time when it was considered worthless for any purpose; it was taken out of the creosote by every tar-distiller in England, whether in London or country, and was now of value for the manufacture of ali-

* This experiment was carried out on a mantelshelf at the Institution of Civil Engineers in August, 1884, with the result indicated by Mr. Boulton.

zarine. The removal was effected by a simple process of filtration; the resulting oils were the green oils, the best part of creosote for timber-preserving, fluent and rich in alkaloids. How could they become "charred oils?"

In the illustration, drawn from a fire-engine, it was forgotten that a fire might break out a second time, and that if a fresh supply of water were not available, the building would be consumed. Carbolic acid evaporated rapidly from timber, and it had been proved that it left no permanent effects behind. When the sleeper was placed in the permanent way the supply of the antiseptic could not be renewed, and the timber would rot if more stable antiseptics were not present in the shape of the heavier oils.

As regarded naphthaline colors Mr. Bamber was also mistaken. They were very successful as a manufacture, and their use was largely increasing. He accused the author of "condemning country oils," and of saying that they "were not good for creosoting timber." In the paper the exact contrary was stated. The author advocated the use of both London and country oils, and he habitually used large quantities of both. What he condemned was the use of oils, whether London or country, which were so manipulated as to contain a large proportion of volatile substances at the expense of the more durable, and therefore for this purpose more valuable antiseptics. Were Mr. Bamber's theories carried into practice, about one-half of the creosote manufactured in England, the enormous bulk of the "London oils," would be excluded from use by the timber-preserver. Nevertheless they were precisely the creosotes which had given the most unmistakably good results, whether, as in the case of the early Indian sleepers, and of the sleepers of the Chemin de Fer de l'Ouest, the percentage of tar-acids had been proved to be small, or whether, as was the practice of the Belgian Government, the tar-acids had been altogether and avowedly struck out of the specification.

In reply to Prof. Voelcker, he desired to state that he had purposely abstained from connecting the names of administrative bodies with the questions of controversy. He was not aware of any specification officially issued by the War Office which bore on this subject; but it was known that the distinguished chemist of that department had been consulted by various administrations, who could have had no other object in view than to obtain the best engineering material. The views of Sir Frederick Abel on all the most important points of a creosote specification were substantially the same as those of Dr. Tidy and of the author. And what the author considered to be the most important points were, 1st, that the presence in considerable volume of the heavier and least volatile distillates, *i. e.*, those distilling at or above 600° Fahrenheit, must not merely be tolerated but insisted upon. That naphthaline, and the other usual semi-solid constituents, should be admitted, provided they were completely fluid at the temperature to which the creosote was raised when injected into the wood. It was known that these views had not been adopted by the Crown agents for the colonies, but he hoped that this discussion might be the means of clearing away many misconceptions. Respecting the point which he considered subsidiary to the other two, although not unimportant, viz., the percentage of tar acids, Sir Frederick Abel, as well as Dr. Tidy, had recently recommended a reduction, and the last word had not been said on this question. Prof. Voelcker was mistaken in thinking that Dr. Tidy had recommended 8 per cent. of carbolic acid. The 8 per cent. was of total tar acids, including carbolic, cresylic, and all other tar acids which could be removed by a specified solution of caustic soda. Dr. Tidy, in his report to the Gaslight Co., mentioned his reasons for not stipulating for a fixed quantity of carbolic acid. Whenever any stated quantity of this body had been mentioned in specifications by English engineers, it had been fixed at one-half of the total tar acids. Hence the quantity had varied from 2½ per cent. to 5 per cent., the latter being the largest quantity of crude carbolic acid which the author had ever known to be required by any specification issued in this country. He might be permitted to express his satisfaction that Dr. Voelcker had recently joined the ranks of investigators into the properties of creosote oils, but he was sure that so distinguished a chemist would be the last to depreciate the experiments and experience of the

numerous chemists and practical men who had placed the results of their labors on record. It could surely have only been by some misconception that Dr. Voelcker recommended an entirely new departure by asking for 10 per cent of carbolic acid in creosotes used for young timber or sap-wood, although he admitted the probable superiority of the heavier oils for timber intended for railway sleepers and other engineering purposes. Dr. Voelcker had not produced the results of any original experiments in support of his views. The typical experiments which he asked for had been tried and recorded; they proved that carbolic acid and the lighter tar acids were not reliable as durable antiseptics for timber. Engineers were familiar with the preparation of young wood and sap-wood as well as with that of older timber. The same creosotes were always used for both, and with complete success. It had been clearly established that the heavy oils preserved sap-wood from decay. It would be remembered by many members of the Institution that the late Mr. Bethell had even advocated the use of young wood in preference to older timber, because the sap-wood absorbed the creosote so readily, and that Mr. (now Sir John) Hawkshaw had combated this idea, not from any doubt of the preservation of young wood, but upon the ground that the engineer must choose for many purposes the kind of timber best adapted for resisting impact or heavy strains. Amongst the numerous successful specimens of creosoted wood which had been exhibited at the Institution during the discussion, and which had been taken from various railways after periods of endurance varying from sixteen to thirty-two years, nothing was more striking than the perfect preservation of the sap-wood, although careful analysis had shown that the heavy oils, and not the tar acids, were the enduring agents of preservation. The allusion of Dr. Voelcker to telegraph poles had elicited much practical information. Nothing could be more conclusive than the evidence of Mr. Preece as to the behavior of the young timber, surrounded by its girdle of sap-wood, which was used for telegraph poles in this country. The author had been responsible for the creosoting of a large portion of the poles alluded to by Mr. Preece; these had as a rule been prepared with the usual London oils. But it was only right that he should state another circumstance. He believed that the success of the poles, creosoted for the Post-office Telegraph Department, was largely influenced by the care taken by that department in the seasoning of the timber. The date of delivery of the poles, landed and stacked at the creosoting yard, was a matter of contract, but there was no fixed date for the creosoting. On the contrary, the engineer did not allow them to be creosoted until he pronounced them to be dry, and ready for the process. Sixteen years ago, at a meeting of the Institution, he had urgently recommended the adoption of some such method for ensuring the proper seasoning of timber. The very interesting and satisfactory evidence of Mr. Bouissou, the Engineer of the West of France Railways, confirmed the experience of Mr. Preece, both as to the satisfactory results of creosoting, and also as to the great importance of seasoning before creosoting; the precautions adopted for the latter purpose by the French company being substantially the same as those of the English administration. With reference to the preparation of telegraph poles, a very valuable paper had been contributed by Mr. William Langdon, M. Inst. C. E., to the Society of Telegraph Engineers, on the 25th of March, 1874. Mr. Langdon had also contributed to this discussion, and had confirmed by his experience many of the views entertained by the author. With regard, therefore, to the observations of Dr. Voelcker as to green or unseasoned timber, the author would add the results of his own long and varied experience in this and other countries, by saying that the attempt should never be made to inject creosote, or any other oily substance, without previously, or at the time of the operation, expelling watery moisture. Timber should not be felled whilst the sap was in it.

As regarded the effects of living organisms, and the introduction of their spores through cracks in the wood, the views of Mr. Carruthers entirely agreed with those expressed by the anthor. But what was the remedy? The botanical aspect of the question had not been lost sight of, from the days when Dean Buckland and others discussed at this Institution the question of timber preparation from that important

standpoint, and it had not been overlooked in the modern systems of injection. Exogenous trees, whose annual growth took place by the formation of concentric layers of vascular tissue added externally, furnished the timber with which engineers had almost exclusively to deal. The softer and younger wood, containing the greatest portion of albumen, was on the outside; it was more liable to decay than the harder portions. It was the chief merit of the system of injecting under pressure that it precisely met this difficulty. The softer parts absorbed more of the antiseptic than the rest, the pressure followed the line of least resistance, the antiseptic fluid gorged the sap-wood, and penetrated to all cracks or shakes. There was but little analogy between this method and the application of a surface coating of pitch, as although he recommended by preference oils of a heavy character, and containing semi-solids, the whole of these bodies were perfectly liquid at 100° Fahrenheit, the temperature to which they were usually subjected at the time of injection. On cooling, they solidified, not on the surface merely, but within the pores of the timber, which they sealed up against the incursion of the agents of decay. Mr. Carruthers had referred to the experiments of the celebrated Dr. Koch. The researches of Koch, and of other German scientific investigators, were very damaging to the claims of carbolic acid as a germicide, and as a coagulator of albumen. In his treatise "Ueber Desinfection," Dr. Koch deduced from his careful and laborious experiments minutely described, that the value of carbolic acid was greatly limited as a germicide, and that for the destruction of spores it was altogether useless, being almost without action; but that it could be used to destroy micro-organisms free from spores. This was when used in a watery solution; still stronger was his opinion as to an oily solution. He stated that in solutions of oil or alcohol, carbolic acid did not exhibit the slightest antiseptic action. To this, the remarks of Dr. Sansom had already pointed. It must be remembered that it was in an oily solution, *i.e.*, dissolved in the tar oils, that carbolic acid was applied to timber. G. Wolffhügel and G. v. Knorre followed up Koch's investigations, and spoke of the inactivity of an oily solution of carbolic acid; of its inferior powers of penetration into porous solids, and of its inferiority in the destruction of fungi. F. Boillat, who followed up the experiments of Koch in the laboratory of Professor Nencki at Bern, found that albumen, when completely coagulated with an excess of carbolic acid, formed no permanent combination therewith. He was able to wash out on a filter the whole of the carbolic acid from the albumen precipitate, after which, upon exposing it to the atmosphere during forty-eight hours, the albumen became putrid. Mr. Carruthers had spoken of the presence of free crystallized carbolic acid in the cells of a small piece of a wooden hurdle. But carbolic acid would not crystallize out of the oils holding it in solution; it could only be obtained in that state of purity by a long and complicated chemical process, and the crystals would immediately liquefy when exposed to the atmosphere. The minute particles seen by Mr. Carruthers were probably naphthaline, or one of the other semi-solids of the higher distillates of coal-tar. The condition of this hurdle corresponded exactly with that of enormous masses of successfully creosoted timber as typified by the samples exposed during this discussion, and the author thought that the final question of Mr. Carruthers had been fully answered by many authorities quoted in the paper.

In reply to Mr. C. de Laune, the author would remark that his paper had a much wider object in view than the mere question of carbolic acid; the presence or absence of that body would not explain Mr. de Laune's difficulty. No honest creosote made from coal-tar, whether "London" or "country" oil, whether with much or little tar acid, contained any ingredient which could injure timber; the only question was, which of those ingredients was most efficacious and most durable. The question as to which was the easiest to put into the timber was of much less importance. Some small pieces of hurdles, &c., had been shown during the discussion, and alluded to by Dr. Voelcker, Mr. Carruthers, and Mr. de Laune; Mr. E. A. Cowper had detected the reason why one had succeeded and the other failed. The first had had plenty of creosote put into it; the others but very little. Mr. de Laune had made a

detailed statement to the author, which was briefly as followed: That he had been in the habit of preparing different kinds of timber of various densities, and frequently in a wet or unseasoned state by boiling the wood in creosote in open tanks and without a thermometer; and that he did not keep the timber in the tanks more than twelve hours, as a longer operation rendered it brittle—a very significant fact. He said that he had not latterly superintended these operations personally, and that he did not regard the process as a scientific one, but thought that it could be carried out by odd hands, old men, or boys. A good many years ago, the author had had considerable experience in preparing timber in open tanks with corrosive sublimate, sulphate of copper, and also with creosote. The time for leaving the timber in the tank, to be injected by the metallic salts in watery solution, which penetrated more readily than creosote, was generally calculated at about twenty-four hours for every inch in thickness of the wood. With the creosoting process it was essential that the water in the timber should be first got rid of; the presence of the water prevented the entrance of the creosote oils. Even with the cylinder-process, where the oil was driven in under pressure, engineers insisted upon the timber being dry, and they weighed it before and after the operation, to check the quantity of creosote injected. With the open-tank system more care, and not less care, was necessary than with the superior apparatus. But soft young timber, if properly seasoned and then subjected to creosote at a moderate heat, could without difficulty be made to imbibe a sufficient quantity of creosote of any kind manufactured in this country. But if the timber was wet, it was not amenable to treatment by creosote in open tanks at a moderate temperature, and if the creosote was raised to a temperature even approaching to its boiling-point, which was about 400° Fahrenheit, it would cause the timber immersed in it to become as brittle as a carrot. Timber should not, under any circumstances, be subjected to a higher temperature than 250° Fahrenheit. It would, therefore, appear that Mr. de Laune's difficulties were to be explained by his methods of operation. He had told the author that he had for many years procured all his creosote from the same works, a small local manufactory, where the tars of the district were distilled. It had been ascertained that the creosotes manufactured at the works in question had not essentially varied in type, whilst even as regarded carbolic acid, if the analysis quoted by Mr. de Laune was correct, the quantity contained in the sample was considerably above the average, although this was a point to which the author attributed but little importance. He was surprised to find, in the report accompanying the analysis alluded to, a statement to the effect that "good creosote should yield quite 75 per cent. of volatile oils (*sic*) containing 10 to 15 per cent. of crude carbolic acid." No creosotes used for timber-preserving, under any specification, had ever been required to contain more than from 2½ to 5 per cent. of crude carbolic acid. The recommendation of "volatile oils" was a mistake which was obvious to all experts; but it might have a bad effect in encouraging the use of some of the worst adulterants, substances sold as creosote which were not derived from coal tar at all. The report, although issued from the laboratory of the Royal Agricultural Society, was signed for, but not by, Dr. Voelcker. The author had understood that Dr. Voelcker was at the time absent owing to illness; he would not therefore have alluded to it but for the fact that this report had been brought so prominently into notice by Mr. de Laune, and that extracts from it had been published in an agricultural journal.

The author had used creosoting for farm purposes, for fences, hurdles, and for many years also, for piles and fences for his wharves. He always used for himself the type of creosote he recommended to others, and it had proved invariably successful in his own case.

The author was asked by Mr. Cleminson why he had not alluded to the process of Mr. Blythe. If by Blythe's process was meant the attempt to introduce the creosote oils, or any part of them into timber in the form of vapor, the subject had been fully treated in the paper. For the operations described as having been carried out for the Compagnie des Chemins de Fer de l'Ouest, the apparatus used was supplied by Mr. Blythe. The experiments of Mr. Seidl were described

by him as having been carried out by "Blythe's process." Engineers in England had recently had an opportunity of witnessing similar experiments at the works of Messrs. Connor, at Millwall. After the dismantling of these works, the author had purchased the greater part of the machinery for the purpose of adapting it to his own processes, so that he had again had an opportunity of studying the question. By slow evaporation, fluids gradually volatilized at temperatures much below their boiling-points. But pressure from their vapors could only be obtained at temperatures exceeding their boiling-points. Thus water gradually evaporated even from a frozen surface, but no tension of its vapor could be produced except at a temperature exceeding its boiling-point, 212° Fahrenheit. The boiling-point of the creosote oils ranged from about 400° to 760° Fahrenheit, that of carbolic acid when separated from these oils being 360° Fahrenheit, and of cresylic acid 390° Fahrenheit. Now, it was well known that timber for the purposes of the engineer was injured and rendered brittle and unsafe at a temperature much exceeding 250° Fahrenheit. How then could those tar products be introduced under pressure into the timber as vapor, whether accompanied or not by superheated steam, without injuring the timber? Either the temperature must be raised above danger point for the wood, or nothing but the vapor of water would be driven into it. This applied to the first part of the process. Of course, if it was followed up by an injection of the creosote oils in the usual manner, this second part of the process covered the deficiencies of the first operation. The presence of any of the components of the tar-oils could be detected in the timber by chemical tests. When specimens of wood had been produced, which had been prepared by the injection of tar-oil vapors in sufficient quantity to have a practical value in the preservation of timber, and at a temperature not exceeding 250° Fahrenheit, the author would be very glad again to give his best attention to this part of the subject.

He was glad to be able to reply to the question of Mr. Lawford, with regard to the Midland Railway Company. In 1866, at a meeting of the Institution, Mr. Crossley, the engineer of that company announced that, although he admitted that creosoting stopped decay, he had given up that process from a calculation of economy based on the assumption, that with very heavy traffic like that which prevailed over the lines of his company, the sleepers were worn out by hard work before they had time to decay. The author would suggest that incipient decay of unprepared sleepers often set in at a very early period of their service, especially through cracks and bolt-holes; the fastenings of the chairs thereupon became loosened, and the mechanical destruction of the sleepers hastened. But Mr. Lawford would be glad to hear that the Midland Railway Company had again adopted creosoting; they had had large quantities of sleeper creosoted during the last few years.

In reply to Mr. Roberts, the author had never found any difficulty in completely saturating the sap-wood with the London oils where the timber had been sufficiently dry. Mr. Coisne's experience with shavings were for the purpose of ascertaining what kind of creosote lasted best, and he effected a complete saturation both with the thin oils and with thick oils. The thinnest oils did not preserve the woody fiber from rotting, even with so good an injection, whilst the heavier oils did. *A fortiori*, the thinner oils would be, by themselves, still more unreliable with the inferior injection carried out in practical operations with timber. It must also be borne in mind that Mr. Coisne did not stop at these experiments, but had confirmed them by twenty years' subsequent treatment of timber on a very large scale, for the Belgian State Railways. The chapter in Mr. Coisne's 1871 pamphlet, upon the choice of creosote oils was a most interesting and practical one.

With reference to the author's process for removing water from the timber at the time of creosoting, the following experiment had been carried out at his works since the paper had been read.

Six square fir-sleeper blocks, each 8 feet 11 inches by 10 inches by 10 inches, saturated with moisture, were cut into 10 inches by 5 inches sleepers. One sleeper, A, from each block was prepared by the new method, the corresponding sleeper, B, from the same block, by the old method, so that in each instance the results

with the two halves of the same log could be contrasted. Care was taken to choose blocks having the heart in the center, and with the texture of the two halves as nearly as possible similar.

From the six sleepers, A, water was withdrawn by the new process to the extent, ascertained by weighing the water, of 120 lbs.; yet the sleepers, when withdrawn from the cylinder after the process was completed, weighed 155 lbs. more than when put in, thus showing that they had absorbed 275 lbs. of creosote. As their total cubic contents were 18.57 cubic feet, their average loss of water was 6.45 lbs. per cubic foot; their average gain of creosote was 14.8 lbs. per cubic foot.

The six sleepers, B, were creosoted by the ordinary process. Being, like the others, very wet, and having no moisture extracted from them, the results of their being weighed before and after creosoting showed an absorption of 116 lbs. of creosote only, or an average of 6.29 lbs. per cubic foot. The separate absorptions of these six sleepers were as followed: 9.04 lbs., 4.52 lbs., 2.9 lbs., 6.13 lbs., 9.36 lbs., and 5.49 lbs. per cubic foot respectively, thus illustrating the uncertain results of creosoting timber when too wet by the ordinary method. They were placed in the cylinder with a charge of ordinary dry sleepers, which took up on the average rather more than 10 lbs. of creosote per cubic foot.

The result with sleepers A was interesting, as it showed that by the new process wet timber could have its moisture at once removed, and a large quantity of creosote injected without difficulty. All twelve sleepers, both A and B, were afterwards cross-cut at 6 inches, 9 inches, 12 inches, and at 4 feet 6 inches from their ends, the corresponding section of A and B being constrasted and photographed. The sleepers A were found not only to have absorbed a large quantity of creosote, but the creosote was much more evenly distributed than was the case with sleepers B.

Might the author be permitted to sum up the evidence which had been produced during the discussion as to the best class of antiseptics for timber? Both engineers and chemists would probably agree with him that after forty-five years' discussion of this engineering problem the time had gone by for dogmatic assertion, unsupported either by experiment in the laboratory or by recorded experience in engineering works. In the paper he had called the germ theory a severe but salutary test for these antiseptics. As a matter of fact, the subject had received valuable elucidation from the labors and discoveries of a number of eminent men, who had studied the physiology of the bacteria. In the application of the remedies, however, the operations of the timber-preserver diverged from those of the physician to the human body. In combating those terrible enemies the bacteria, which were pathogenic to animal life, the great difficulty was that many of the remedies effectual against the bacteria intefered with the vital functions of the patient. On the other hand, the physician could repeat remedies whenever the malignant symptons reappeared. Therefore antiseptics, more or less volatile, were sometimes more useful to the physician than others of a more permanent character, because they did not accumulate in the system of the patient. In preserving timber, the problem differed materially. The vital functions of the plant had ceased; stronger poisons, and substances which clogged up the cells and tissues, could be employed, provided always that they were of such a nature as not to injure the structure of the wood. But the remedy must be applied once for all. In the majority of cases where timber was once placed in engineering works the supply of the antiseptic could not be renewed. Therefore the very first condition was that the antiseptic should be of a permanent constitution. Let this rule be applied to the evidence offered during the discussion. Antiseptics for timber had been described: 1st, as coagulators of albumen; 2d, as germicides; 3d, as sterilizers, rendering the cells of the wood unfit for the development of fungi or bacteria; 4th, as germ-excluders, closing the entrances against the intrusions of the enemy.

Was not too much value still attached by some to the coagulation of the albumen in wood? Albumen formed an extremely small portion of the wood; in fir it varied from 0.5 to 0.9 per cent. Those parts of timber containing the smallest portions of albumen were nevertheless liable to decay; the mere coagulation of

the albumen did not protect the bulk of the timber from destruction. Did coagulation preserve even the albumen itself from destruction? Sansom, Angus Smith, and other authorities found that it did not. The author took a hard-boiled egg, a very complete specimen of coagulated albumen, removed the shell, and exposed it to the sea breezes on a high point of the Atlantic shore of the island of Mull. In a few days signs of putrefaction were visible; in eight days the albumen was coated with various species of micrococcus, cromogenes and other agents of destruction. The egg had become a mass of corruption. Coagulation had not protected albumen from putrefaction. What was the result when coagulation was produced, not by heat, but by the action of an antiseptic body? Did not the result depend mainly, if not altogether, upon the germicide properties of the antiseptic, and upon its abiding presence? Or, thus produced, did coagulation *per se* effect a new combination with permanent results? In the case of carbolic acid, a host of investigators said, No. Their experiments appeared to prove that carbolic acid was volatile in the air, soluble in the water, and that its compounds were not stable. Boillat, in his experiments, realized the extreme conditions desired by those theorists who thought that carbolic acid had a permanent effect upon timber; he produced a perfect coagulation of albumen with an excess of carbolic acid. Yet a mere washing with water removed the whole of the carbolic acid, and the albumen putrefied on exposure to the air. Carbolic acid, therefore, would appear to have had no permanent effect upon albumen. The author agreed with Dr. Bernays that if coagulation by carbolic acid were desirable, 2 per cent. of that body might be retained; but having in view the foregoing evidence, what was the value of the coagulation theory at all as applied to timber-preserving? There had been some idea that carbolic acid lingered in the timber in some unrecognized form. The author had had occasion to test sleepers a few weeks after creosoting: if this were done before the carbolic acid had time to evaporate, it could be found in the wood by the ordinary tests, and a quantitative analysis made. On the other hand, Dr. Tidy, who was not unwilling to find it in combination, had searched for it after twelve months, and had not found it by the ordinary tests, that is, in sufficient quantity to have any practical result. But whenever it was present there were tests subtle enough to detect it, even in such infinitesimal quantities as to have no practical value, as was evidenced by the experiments of Mr. Greville Williams. Notwithstanding theories and experiments, did carbolic acid, when put into timber, do any good there? Mr. Coisne, Mr. Greville Williams, and the author, not only never found it to have contributed to the success of old creosoted timber, but Mr. Coisne's experiments went further still. He injected woody fiber with light oils and an excess of tar acids, and the woods rotted, whilst the woods creosoted with heavy oils, and without any tar acids, were preserved.

There was one point respecting which there had been a consensus of opinion on the part of all who had taken part in the discussion, namely, that for the preparation of timber, creosoting had been undeniably more successful than corrosive sublimate, sulphate of copper, or chloride of zinc. Could this be at all due to carbolic acid? How was this possible, when a host of authorities proved that carbolic acid was less permanent in its effects than the three metallic salts alluded to, and very considerably less powerful as a germicide than corrosive sublimate or sulphate of copper. In a valuable work upon bacteria by Magnin and Sternberg there was a long list of antiseptics, with a statement as to their comparative potency as germicides, compiled from the latest authorities. Carbolic acid was low in the scale. Dr. Sternberg gave the strength of solutions of different kinds which had been found efficacious in preventing the development of the septic micrococcus, the following amongst many others:

Corrosive sublimate...	1 part in	40,000
Sulphate of copper....	1 "	400
Chloride of zinc.	1 "	200
Carbolic acid.....	1 "	200

These were in watery solution. To this must be added the statements that in oily solution the antiseptic power of carbolic acid was diminished, according to Sansom and Angus Smith; altogether it was nil according to Koch. It was evident that there was a vast accumulation of scientific evidence in confirmation of the

continually reiterated statements of those practical men, who had had the largest and longest experience in preparing timber, to the effect that it was not to carbolic acid, but to other substances contained in the tar oils, that the superiority of the creosoting process over the other three methods was due. Mr. Lowe was well known as one of the highest scientific and practical authorities upon the tar acids, and he had given much valuable information in his communication to the Institution. On the other side, the absence of evidence was even more remarkable than many of those interested in this subject would perhaps have anticipated. No chemist had brought forward even a laboratory experiment in proof of any permanent effect of carbolic acid upon albumen. No practical man had produced a proof that that substance had had any lasting effect upon timber. The author submitted that the claims of carbolic acid as an antiseptic for timber had not been proven.

What, then, were the substances in the creosote oils which had insured the superiority of that process over the others? If the author were asked the question, he would remark that the object being the prolonged preservation of timber, antiseptics should be chosen which remained longest in the timber. That the different constituents of the creosote oils, showed a gradation from the lightest and most volatile bodies at the carbolic, or left-hand end of the scale, up to the least volatile and most permanent bodies at the right-hand end. Divide the bulk of the oils roughly in half. Would the constituents of the right-hand half of themselves insure the preservation of the timber? Yes, excellently well. They contained germicides and solidifying materials; they were both sterilizers and germ-excluders; they would not evaporate, except at an enormously high temperature. Nevertheless in their united bulk they were perfectly fluid at a temperature of 100° Fahrenheit; they were insoluble in water; they could be injected into timber, in quantity exceeding the maximum which any engineer had as yet required. Would the other, or left-hand part of the group, taken by themselves, preserve timber? Much less perfectly, as they were more volatile. Would a still further fractioning to the left, if it were practicable, insure a better result? Not so, but a worse one still; for the lightest oils, which contained the greatest portion of the tar acids, were, like the tar acids themselves, the most volatile portions of all.

The author trusted that he had made clear his reasons for specially objecting to large percentages of tar acids. Take an honest heavy cresote, free from adulteration, free from mutilation, containing, say, 5 per cent. of tar acids. If this sample were refused because it did not contain 8 or 10 per cent., the tar-distiller was induced to remove a large portion of the heavier constituents of the bodies to the right hand of the scale, in order to make the proportion of tar acids larger in the portion remaining. He believed that those heavier portions were the best. He thought that, provided the oils were sufficiently fluid at the temperature at which they were injected, there should be no restriction as to maximum specific gravity or maximum boiling-point. If larger and stronger doses of germicides were desired, it would be far better to put them into the wood in the shape of corrosive sublimate or sulphate of copper, in addition to the heavy oils. This could be done by a double process of preparation, with respect to which he had been lately experimenting.

Timber preserved by antiseptic treatment was an engineering material competing with other materials, both as to price and durability. Members of the Institution would appreciate the endeavors of the author to emancipate an important industry from the effects of any theories which, themselves unproven, might stand in the way of improvement, either as to diminished cost or increased efficiency.

Strikes and rumors of intended strikes are on the increase in Germany, and some of the revelations as to the "starvation" wages that prevail in certain branches of labor are somewhat startling. Thus, the linen weavers of Erdmannsdorf, a village in Silesia, have ceased work in an attempt to secure an advance of about 20 per cent. on their wages, which average six and a-half marks per week for twelve hours' work per day, and seldom or never exceed 9 marks, equivalent to as many shillings.

PORTLAND CEMENT.

By HENRY FAIJA, M.I.C.E.

From "Iron."

ALTHOUGH a great deal has from time to time been written about Portland cement, the author is bold enough to think, and also to say, that very little is known about it, except to those intimately connected with its manufacture or who happen to be large users, and have, therefore, been obliged to make themselves thoroughly acquainted with the subject. In general, little is known about it, so little, indeed, that the specifications submitted to manufacturers are often absolutely impossible to work to; and even supposing they could be, the result would probably be a material of which at present nothing is known; it might, in some instances, do the duty of cement, but even that is doubtful, and it certainly would not be a Portland cement.

The error into which most of the drafters of these specifications fall is that they have heard or read that a cement should be extremely finely ground —the finer the better—also that it should be heavy; therefore, by inference, the heaviest weight is specified in conjunction with the finest grinding, and these, unfortunately, are properties which cannot both be obtained in the same cement. It is not at all uncommon to see in a specification that the cement shall weigh 120 lbs. to the striked bushel, and be so finely ground as to leave only 10 per cent. of residue when sifted through a sieve having 4,900 holes (70 by 70) to the square inch. Now, it would be impossible to produce a cement which would comply with these requirements. It should be remembered that the finer a cement is ground the more bulky it becomes, and consequently weighs less per striked bushel. The weight given above, 120 lbs. per striked bushel, would probably correspond with a fineness, or rather a coarseness, of a residue of perhaps 30 to 35 per cent. on the 70 by 70 sieve, and 20 per cent. on the 50 by 50 sieve; while the fineness mentioned, 10 per cent. on a 70 by 70 sieve, would correspond to a weight of perhaps 106 to 110 lbs. per striked bushel. Of course, the bushel measure is to be filled by one or other of the recognized methods, so that it shall be filled as lightly as possible, and in no way touched or shaken until the cement in it has been struck level on the top. For the practical purposes of the user, however, the weight of the cement is now acknowledged to be of very little service in assisting the determination of its value. It is a test which originated with the manufacturers, and to the manufacturer working always with the same raw materials, it is of use, for it enables him to form an opinion as to the degree of calcination to which the cement has been subjected, the harder burnt producing the heavier clinker. But to the user the weight is no guide whatever, since peculiarities in the raw materials, without being detrimental in any way, may produce, on calcination, a heavier or a lighter cement. In the author's testing room a weight test is carried out if it is specially asked for, but as for being any guide or assistance in forming an opinion of the value of a cement it is absolutely useless, and, except when asked for, he has not carried out a test for weight for many years.

Another matter in which specifications often err, is in the wording not being sufficiently explicit. The following is a very favorite sentence, which, from its popularity, must be derived from some published work or specification of which the author is not cognizant; it is very misleading, and has, within his knowledge, led to more than one dispute. It is: "The breaking weight per square inch seven days after being made in a mould, and immersed in water during the interval of seven days, shall be," &c., &c. Now, the wording evidently reads that directly the cement is gauged and filled into the briquette molds, molds and all are to be put in water, because it would be evidently impossible to remove the briquettes from the molds until they were set, a proceeding directly contrary to practice and to reason, but one which the author has seen insisted upon.

Again, if the briquettes are not placed in water until twenty-four hours after they are gauged, which is the usual practice, it would become an eight days' test, *i. e.*, briquettes gauged on a Monday would be tested on the Tuesday week, leaving seven clear days in between, which, of course, is in favor of the manufacturer, because the cement has one day more to acquire its strength; but the wording is altogether vague and indifferent, and, worst of all, read it any way, it is not in accordance with ordinary practice, and comparative results between different samples of cement cannot be obtained if the same means and procedure are not adopted in every instance.

Specifications are matters which affect the manufacturer more than the user; the user has a right to ask for what he likes, and if the manufacturer undertakes to supply him, he is bound by his contract to do so. It is, therefore, surprising that manufacturers have not before now drafted a specification, and formulated a manner of procedure for carrying out a cement test which would satisfy the requirements of a good cement, and which they would be prepared to work up to. To enumerate a tithe of the absurdities which appear in cement specifications would take up more time than can be spared, and their consideration would not be profitable. Having given two instances, it may be at once stated that a cement specification is about the easiest and shortest to write, when once the nature of the material is known, and the particular requirements of the case ascertained. The object of testing cement is to determine its value as a constructive material; the object of the specification is to define the value which is required, and unless the latter is clear and explicit it is hardly possible for the manufacturer to comply with it, or for the test to be of much value.

So far as experience goes, the ultimate strength of Portland cement is not known, nor for practical purposes is it necessary that it should be. A good Portland cement is known to continually increase in strength for several years, and there is no reason to suppose that it will —within the limits, at all events, of the life of other constructive materials—in any way deteriorate. It must, further, be borne in mind that cement is always too strong for the purpose for which it is to be used, and that it is always weakened—or, as it were, thinned out—by the addition of sand or other ingredient. If, therefore, the strength of the structure is determined, as well as the proportion of cement to sand, or aggregate, which is to be adopted, the required strength of the cement may be ascertained—with this proviso, however, that the size, nature, and other properties of the sand or aggregate will materially affect the strength of the structure; therefore if only an indifferent quality of sand is obtainable, either more cement must be used, or a greater strength demanded. The simpler way, however, is not to specify or ask for a cement which is not usually made; take the ordinary article of commerce, and thin it out with as much or as little sand or aggregate as meets the requirements of the case. The matter of specification is thus reduced to the very simplest form, and the object of the test is to ensure the delivery being in accordance with the ordinary demand.

The only three properties of a cement which it is necessary to know in order to determine its constructive value are: 1st, its fineness; 2d, its tensile strength; and 3d, its soundness. Though usually placed in this order, it would be more rational if their order were reversed, for, if a cement is unsound it is evidently useless, while its fineness and tensile strength merely mean the use of so much more or less sand. Taking these, however, in the order in which they are here placed, the author will consider, first—fineness. A mortar or concrete is composed of a certain quantity of inert material called "aggregate," which is bound together by a cementing material, the matrix, and it is evident that, to secure a sound mortar or concrete, it is essential that each piece of aggregate shall be entirely surrounded by matrix, so that no two pieces are in actual contact. It is hardly necessary to explain that the finer a cement is ground the greater surface will the same weight cover, so that the finer a cement is ground the more economical will it be to use. But there are other reasons why it should be well ground. Cement, until ground, is a mass of partially vitrified clinker which is not affected by water, and which has no setting powers; it is only after it is ground that

the addition of water induces crystallization. It therefore follows that the coarse particles in a cement have no setting power whatever, and may, for practical purposes, be considered only as so much sand, aggregate or practically an adulterant. So far, therefore, it appears that an impalpable powder should be asked for; but here the question of cost and economy steps in. The manufacturer will say that it is possible to grind cement to any degree of fineness if paid for, but there is a degree of fineness which is economical, *i. e.*, when it becomes cheaper to use more cement in proportion to the aggregate than to pay the extra cost of additional grinding. With present appliances and means of grinding, that point is, in the author's opinion, reached when the cement is ground to such a degree of fineness that when sifted through a sieve having 2,500 holes (50 by 50) to the square inch, it shall leave a residue of not more than 10 per cent. by weight. Cement ground to this fineness will leave from 19 to 21 per cent. of residue on a 4,900 (70 by 70) sieve, and practically nothing on a 625 (25 by 25) sieve, and most manufacturers grind now to that degree of fineness.

Secondly, tensile strength.—It would be useless to recapitulate the numerous experiments as to the strength of cements which have been made from time to time by the author and others, and published in the Proceedings of this and other institutions. The deductions which have been arrived at by an examination of these tests are that the quick-setting cements and those which acquire great strength in a short time, or within the limits of test, do not, as a rule continue to increase in strength after a few months, and that after they have attained their ultimate strength they have a tendency to fall off. The extent of this depreciation has not been conclusively proved, but it is evidently not desirable to use a material which may, after a lapse of time, seriously deteriorate in strength. It is better to use a cement the strength of which is somewhat less at the early dates, but which it is known will continue to increase for an indefinite period. Such a cement is secure when it is slow setting, and when tested at different dates shows a marked increase in strength with the age of the briquettes. It was formerly the practice to demand the strength of a cement at the expiration of twenty eight days from gauging, but such a lapse of time is impracticable, because in most cases the cement would be in use before its strength was ascertained. The author has, therefore, entirely abandoned it, except as a corroboration of an opinion formed by tests at shorter dates. Seven days is, as a rule, the limit of time available for carrying out a test, but the strength at only that date would not enable a definite conclusion to be arrived at as to the value of the sample, and it is therefore desirable to carry out a test at an earlier date Three days is customary, and the ultimate strength of the cement is judged by the increase in strength between those dates. Experience shows that a cement need not have great strength when only three days old, but that it should show a marked increase between that and the seventh day's test. At the same time, in order to secure a material which will be capable of exerting, or rather of acquiring, a certain strength in a fairly short time, a minimum strength at both dates must be specified. The author is of opinion, and he is well supported in it, that a cement should carry, at least, 175 lbs. per square inch at the three days' test, and should show an increased strength of at least 50 per cent. at the seven days' test, but that the minimum strength at seven days should be 350 lbs. per square inch.

These strengths, of course, refer to the cement when gauged and treated in the ordinary manner, the briquettes being of the most approved form to resist a tensile strain. The form of briquettes which gives the best results is, so far as its form to resist a tensile strain is concerned, that designed by Mr. Grant, of the Metropolitan Board of Works, although it is slightly varied in other respects. Independently of the form of the briquette, there are, however, several other points which materially affect the results obtained, and though it is hardly within the province of this paper to give a full description of the means of gauging cement, still it would be incomplete without referring to them. They are—the amount of water used in gauging; the expedition with which the gauging is accomplished and the briquette formed; the

time which is allowed to elapse between the gauging of the briquette and placing it in water; and, lastly, the rate of speed at which the strain is applied to the briquettes when being tested.

There is no doubt that a briquette which is put in water directly it is set, will show a greater strength than if it is left in the air for a considerable time. But in adopting this practice, it is difficult to decide the exact time when a cement is set. If put in water too soon the strength is deteriorated. For this reason, and to secure uniformity in procedure with all cements, whether quick or slow setting, it is usual to place the briquettes in water twenty-four hours after they are gauged.

The second point really means skill in gauging, as it is evident that anyone accustomed to the work will be able to bring the cement to a proper consistency with less water, and in a shorter time than a novice, and the less water used and the quicker the operation is performed, the better will be the results. To obviate this difficulty as far as possible, the author some years ago devised a small machine in which to gauge cement, and he has since used it continuously. It is extremely simple, and consists of a stirrer revolving around its own axis in one direction, and around the pan the reverse. To use it, a weighed quantity of cement is placed in the pan, and the exact quantity of water (which, by previous experiment that particular sample of cement has been found to require) is put in at once, and the handle turned until the cement is reduced to the proper consistency. It is then turned out of the pan on to the gauging slate, and beaten up with the trowel into a convenient form and placed in the molds. It is then lightly rammed and shaken so as to remove all air bubbles, smoothed off on the top, and not touched again until the next day, when each briquette is marked and dated, taken out of the molds and placed in water, where again they are not touched until they are to be tested. In determining the strength of a sample it is well to take the average of five briquettes at each date, as from unforeseen causes one might not fairly represent the strength of the cement, and, with this object, it is well to have the molds arranged in nests of five.

In a paper which the author presented to the Institution of Civil Engineers last year, and which was published in their *Proceedings*, vol. lxxv., he gave the results of a large number of experiments he had made, with the view of determining the variation shown in the strength of a cement, due to the difference in the speed at which the strain was applied, and within the limits at which it would in practice be possible to apply the strain—viz., 100 lbs. per second, and 100 lbs. per 120 seconds. There was a difference in favor, of course, of the quicker rate, of more than 23 per cent. Without claiming any very great sagacity for carrying out this experiment, for in testing other materials the rate of speed is specified, the author believes he was the first to carry the matter out to a practical issue, for though some thought the strain should be applied quickly, and others slowly, there seemed no very clear idea of what was slow and what was quick. In order to arrive at a happy medium in this respect, the author is of opinion that a rate of speed of 100 lbs. per 15 seconds is a fair one. It is one he has used for a number of years, even before he made the above-mentioned experiments, and he sees no reason to alter it. Of course, any speed may be adopted, but if the usual speed—*i. e.*, 100 lbs. per 15 seconds—is not intended, the speed required should be mentioned in the specification.

With regard to the third point—soundness of the cement—it is to be observed that, naturally, if a cement is not sound—if it contracts or expands, cracks, or what is called "blows," it is absolutely useless, and it is desirable to determine in as short a time as possible whether or not a cement possesses this characteristic. Because a cement carries the required strain at the three and seven days' tests, it should by no means be assumed that it will not blow. Many cements are slow in developing this characteristic, and may even carry through the twenty-eight days' test successfully and blow after that. It is usual to make pats of cement on pieces of glass, or other non-porous material, to place them in water directly they are set, and examine them each day, and see if any cracks show themselves, first of all, around the edges. Now, with all deference to those who

carry out this experiment. The author regrets to say that he has never been able to arrive at any satisfactory conclusion from the examination of the pats. If the cement is really a blowey cement, the briquettes will develop the cracks as well as the pats, and, therefore, the pats are of no use; if the pats blow and the briquettes do not, and carry the desired strain when tested, the inference is that the pats were put in the water too soon, and before they were set, and thus the blowing is accounted for. Cements vary so much in their behavior when placed in water before they are set. A slow-setting cement, which should generally be a well made one, will fall entirely to pieces if put in water before it is set, and will afterwards harden up in the form in which it has fallen, while a quick-setting one, *i. e.*, an overclayed cement, will set quicker and harder if put in water immediately on being gauged; in fact, many cements may be gauged and made up into a ball and put in water at once, and will not alter in form nor disintegrate in the slightest degree, but will harden up as quickly, or even more quickly than if left in the air. To get over these difficulties the author uses in his testing room an apparatus by means of which he is able to determine the soundness of cement in twenty-four hours, or even less. He brings it forward with some diffidence, for the reason that though he has used it now continuously for some years, and has never been wrong in an opinion formed by its indication, in the hands of the unskillful or careless much harm might be done, and many good cements condemned; in fact, it is more suited to a laboratory than to a testing room. It is well known that a moist heat is conducive to, and accelerates, the setting of cement. A dry heat, although it may make a cement set quicker is not beneficial, because it tends to make it friable by removing from it the water which is required for its perfect crystallization. When a cement is set it may be materially hardened by immersing it in a warm silicious bath, and the combination of the moist heat and the warm silicious bath has formed the subject of a patent by the author for accelerating the hardening of concrete. Now, the author's method of determining the soundness of a cement is based on the theory that by accelerating the setting and induration of a sample, all its properties, good and bad, are developed in a short time, and hence the result, which, in the ordinary course of things, would not be known for, at all events, some days, and it might be weeks, is determined and ascertained in a comparatively short time. Following out this idea, the apparatus was designed, which consists of two vessels, the one inside the other, the annular space between the two being filled with water, which tends to maintain the inner one, which may be called the working vessel, at a fairly even temperature. Underneath is arranged a lamp or gas burner, which is so regulated that the water which fills the working vessel is maintained at an even temperature of 110° Fahr. Inside the working vessel, above the water line, are arranged ledges, on which the pats of cement are placed directly they are gauged, the slight evaporation from the water tending to keep the upper portion at a moist temperature of about 95° Fahr. A thermometer is placed in the water, and projects through the cover, so that the temperature may at all times be ascertained. It is also usual to insert a maximum thermometer, so that any undue increase in temperature through varying pressure of the gas may be ascertained. If, during the night or temporary absence, it is found that the temperature of the bath has risen much above 120°, the fact of a pat having blown is not taken as an indication that the cement is unsound, but that excess of heat has been the cause. Hence the diffidence of the author in placing the apparatus in unskilled or careless hands. Any sound cement treated in this manner will stand 120°—some much more, even as much as 140°; but many really unsound cements will stand 100° to 105° for the twenty-four hours without showing decided signs of blowing. The steamer, as it is called, for want of a better and more appropriate name, having been started and regulated, the pat of cement on a glass slab is placed on the rack directly it is made, where it is left for four or five hours. The exact time is not of much importance, but the pat should be perfectly set. It is then taken off the rack and put in the warm water underneath, where it is left for about twenty-four

hours from the time of its being made. If it is then still hard and tight on the glass there is no doubt that the cement is perfectly sound; if even it is off the glass, but presents a smooth glazed surface, the cement may be considered sound. If, having left the glass, it is cracked on the under surface, the cement is looked upon with suspicion, and another trial is made, as the seeming unsoundness of the pat may possibly only be due to the extreme freshness of the cement. If the pat is thoroughly blown, there is not much to say in favor of the cement.

The specification which is deduced from the foregoing remarks is extremely simple, and is as follows:

1. *Fineness.*—To be such that the cement will all pass through a sieve having 625 holes (25 by 25) to the square inch, and leave only 10 per cent. residue when sifted through a sieve having 2,500 holes (50 by 50) to the square inch.

2. *Soundness.*—That a pat made and submitted to moist heat and warm water at the temperatures and in the manner already described, shall show no signs of blowing in twenty-four hours.

3. *Tensile strength.*—Briquettes which have been gauged, treated and tested in the prescribed manner shall carry an average tensile strain without fracture of at least 175 lbs. per square inch at the expiration of three days from gauging, and those tested at the expiration of seven days from gauging shall show an increase of at least 50 per cent. over the strength of those at three days; but the briquettes broken at the seven days' test shall carry an average tensile strain without fracture of at least 350 lbs. per square inch.

Such a specification meets all requirements, and satisfies the peculiarities of nearly all cements, except, perhaps, the very quick setting ones, for which a slight variation in the tensile strength and percentage of increase between the dates named would have to be made.

The present paper would hardly be complete without a few remarks as to the manner of taking samples for testing. There are certain precautions to be observed, as well in the interests of the user as in those of the manufacturer, and it is always desirable, in order to avoid a multiplicity of tests, to obtain a fair sample of the whole delivery. If the sample is taken from bulk in the manufacturer's warehouse, at least seven or eight samples should be taken from each 100 tons. They should be taken from different parts of the heap, and not from the surface or outsides, but from about a spit down. These should be taken to a clean part of the warehouse floor and thoroughly mixed by being turned over and over with a shovel, and sufficient of this (about 10 lbs. is enough) should be taken for testing. If in barrels or sacks, one or two samples should be taken from each hundred and mixed in the same way. The samples in this case should be taken well from the center of the barrels or sacks, as the outsides may have been damaged by exposure or other causes over which the manufacturer has no control. Samples in this way should be taken from about each 100 tons and tested separately. It is always desirable, when time permits, and especially when sampling from bulk, to spread the cement out on a tray for a day or two to allow it to cool before commencing the test. Theoretically, a Portland cement may be used directly it comes from the millstones, but in practice this is not found to be the case, hot, fresh cement being always more or less dangerous to use, and the remark that applies to the cooling of the sample previously to testing also applies to the treatment of the cement before using it. As soon as it arrives at its destination let it be turned out of the sacks or barrels on to a cool and dry warehouse floor, and there let it remain as long as possible, turning it over occasionally. Unless it gets wet or damp, it will not deteriorate in quality, but will improve. It may perhaps be said that the manufacturer should deliver the cement properly cooled and fit for use, but independently of the purely commercial reasons which suggest to the manufacturer the advisability of not keeping too large a stock, the amount of warehouse space which he would require if this were the practice, would be out of all proportion to the size of the rest of the works.

In the early part of the paper the author used the word "adulterant" when referring to the coarse particles of a cement. He refers to it again, because the Cement-Makers' Union in Germany has

lately had a paper submitted to it, being the result of certain experiments by Dr. Fresenius, made with the object of deciding whether or no a cement is adulterated. Dr. Fresenius says that cement has lately been much adulterated with a material so similar to what cement should be, that even chemical analysis fails to detect it, and he therefore submits to the Union a number of tests, or rather chemical processes, by which such adulteration may be detected. The author is glad to say that in his practice he has never yet met with an adulterated English cement, any shortcomings in a cement having been due in all cases to more or less faulty manufacture, or to the use of improper raw materials. As every matter of chemical research, or it may be said of testing, connected with cement which emanates from Germany has great weight with many in this country, the author thinks it but right that he should say this much in honor of the English manufacturers who have not had the opportunities of the technical education which is so general in Germany, and which seems, in this particular manufacture, at all events, to have enabled the pupil to circumvent the master.

So far as regards these special tests advocated by Dr. Fresenius, the author is not in a position to say whether they are of much value; he is at present trying them in his laboratory, and may be able to say more about them on a future occasion. *Prima facie*, it would seem that they are not required, for evidently if the adulteration is carried to any great extent, the ordinary tests for tensile strength would detect it. However, the user may be quite satisfied that when using English cement, he may have a bad or an unsound one, which the tests advocated in this paper will detect, but he is not at all likely to meet with an adulterated one.

THE RELATIONS BETWEEN ENGINEERING AND ARCHITECTURE.*

By W. GOLDSTRAW.

From "The Building News."

WHAT *are* the relations between engineering and architecture? We may take them to be, on a reduced scale, the relations between science and art. Indeed, it is scarcely an exaggeration to say, that the numerous objects brought together in the great museums of science and art are but illustrations of engineering and architecture in the widest sense, with their accessory arts and sciences. These two great departments of knowledge and skill are complementary to each other, as the masculine and feminine natures, strength predominating in the one and grace in the other; and although they have many characteristics in common, they have each their special place and functions. It may be useful, therefore, and will, at least, be interesting, to essay a brief consideration of their relative positions and values as honorable and lucrative professions. In order to get clear ideas on the subject, let us try to attach a definite meaning to the expressions employed. What is engineering? For an answer to this question we naturally turn to the great Society which is the recognized embodiment of all that is foremost in the engineering world. Now, the Charter of the Institution of Civil Engineers contains a lengthy attempt at a definition of "that species of knowledge which constitutes the profession of a Civil engineer." It is there described as "the art of directing the great sources of power in nature for the use and convenience of man, as the means of production and of traffic in States, both for external and internal trade." This is the gist of the definition, which then goes on to specify five main branches of "the art," "as applied (1st) in the construction of roads, bridges, aqueducts, canals, river navigations and docks, for internal intercourse

* A paper read before the Liverpool Engineering Society.

and exchange; and (2d) in the construction of ports, harbors, moles, breakwaters and lighthouses; and (3d) in the art of navigation by artificial power for the purpose of commerce; and (4th) in the construction and adaptation of machinery; and (5th) in the drainage of cities and towns." This definition is not very clear, and not quite comprehensive. There is no mention of railway, mining, hydraulic, gas, or electric engineering; and it is only with great difficulty that these important branches of the subject can be brought within the scope of definition. The fact that some of them had not been developed at the date of the Charter is not a sufficient answer to the objection, and even this explanation does not account for the omission of mines and waterworks. Too much stress is laid on using the power of Nature "as the means of production and of traffic" for purposes of *trade*, whilst, at the same time, what has come to be "sanitary engineering" is distinctly included. These considerations incidentally show the wisdom exercised by the founders of the Liverpool Engineering Society in adopting so expressive and practical and comprehensive a title, and in admitting to its membership "engineers of any branch of the profession." It is *engineering* with which we are concerned, not any one branch of it, not even such an extensive one as that known as *civil* engineering. And, without venturing on any exact definition, it will perhaps be sufficient to say that engineering is that entire system of knowledge and skill which comprises all mechanical pursuits so far as they supply the material wants of men. What is architecture? "The art of ornamental and ornamented construction," chiefly as applied to buildings and such-like structures. Building, considered as a science, is clearly an important branch of engineering. And, as architecture is chiefly concerned with building, it follows that engineering is, in one aspect, an essential component of architecture, though the science may subsist without the art. In other words, whilst there can be no architecture, without engineering, there may be engineering without architecture. Therefore we are led to the conclusion that architecture is the development and refinement of an important branch of engineering. Thus, in a certain sense, the profession of architecture is, in its higher capabilities, more honorable than that of engineering. For it is disparaging to any particular architect to say of him that he is merely an engineer, since this is equal to saying that, so far as his artistic abilities are concerned, he is not an architect but a builder. And, on the other hand, it is not regarded as a discredit to an engineer to pronounce him to be no architect. The sum of these considerations is that engineering construction is scientific and utilitarian, whilst architectural construction is not only scientific and utilitarian, but is also ornamental, and even artistic or beautiful. This distinction is not exact, and cannot be made so. At the same time it is practically convenient, and expresses the principal facts. Having cleared the way thus far, it may be profitable to inquire (1st) whether the relations between engineering and architecture are fixed and unalterable, and (2d) whether, if they are not permanent, it is desirable that they should be modified in practice. Now, when we endeavor to ascertain whether the relative positions of these pursuits are stable or not, we have to glance at their history. With regard to engineering, many of the mechanical arts and sciences comprised in it are so modern in their origin and development that they can hardly be said to have a history. Engineering, as a profession distinct from architecture, is a thing of to-day. Architecture also, as a "profession," is comparatively modern. But engineering and architectural pursuits have occupied men's talents and energies from the earliest times. They were always formerly practiced by the same persons. The new feature is that they now diverge into separate channels. This is, of course, only a phase of the 19th-century system of the division of labor. And as that principle is constantly operating in all departments of knowledge and skill, and must go on dividing and subdividing every trade and profession as the knowledge and skill grow more exact and positive, it appears quite probable that engineering and architecture, as now understood, will never again be practiced together to any great extent by the same persons at the same time. But, as we have said, it was not always so. And there is no reason in the essential nature

of things why it should be so now. Chiefly, what may be said is that the requirements of latter-day science have made it inconvenient and difficult for any one man to follow at once engineering and architecture equally well. And, secondarily, it must be acknowledged that modern ideas as to the province of the architect have much to do with the severance which we are considering. It appears to be taken for granted that the work of the architect should be confined entirely to buildings. But the modern historian of architecture (Fergusson) maintains that "there are no objects that are usually delegated to the civil engineer which may not be brought within the province of the architect. A bridge, an aqueduct, the embankment of a lake, or the pier of a harbor, are all as legitimate subjects for architectural ornament as a temple or a palace. They were all so treated by the Romans and in the Middle Ages, and are so treated up to the present day in the remote parts of India, and wherever true art prevails." Now, this is but equal to saying that in many large public works there is room for the engineer and the architect alike, or, at least, for their special talents. The architect should have some advantage, however, in the fact that the scope of his calling is wider, if fairly regarded, as it includes much that is simply engineering. But if the principle of the division of labor is to run to its natural issue, architecture will be considered as supplementary to engineering, not subordinate, perhaps, but rather superior, in the sense of its being the application of embellishment to the naked structure, or the incorporation of ornament into it, or the tasteful disposition of its parts. For, as Fergusson says, "where the engineer leaves off, the art of the architect begins. His object is to arrange the materials of the engineer, not so much with regard to economical as to artistic effects, and by light and shade, and outline to produce a form that, in itself, shall be permanently beautiful." If these considerations are allowed to have due weight, they tend to show that, although the connection between engineering and architecture has become relaxed, it is quite capable of being drawn tighter, and that the two branches of construction are by no means firmly settled apart, notwithstanding the force of convenience and custom, and the general disregard of art and beauty. It is, therefore, practically possible that the engineer should be more of an architect, and that the architect should be more of an engineer. We may now turn to the second part of our inquiry. For, if we have shown that the relations between engineering and architecture are not fixed and unalterable, the question naturally follows, Whether it is desirable that those relations should be modified in practice. Now, the answer to this question will depend upon another, which has already been touched at some points: How does the present arrangement work? Take, for instance, the specially modern case of a railway and its appurtenances. The actual railway itself, both as to the surveys for its course and the planning and construction of its different parts, is the work of the engineer. The tunnels and bridges are as properly assigned to him as are the track and signals. And, in many instances, the station buildings are regarded as coming equally within his province to design. If, however, the buildings are of great extent, and occupy an imposing site in a large town, they are sometimes put into the hands of an independent architect, with a view to insuring, amongst other things, a fairly artistic effect. This is constantly the case when the station buildings are connected with an hotel placed so as to mask the station itself. And, although the smaller or country stations are frequently designed by the engineers of the company, there are instances where high-class architectural firms are employed to take in hand everything in the nature of buildings connected with these stations, including even the roofs of iron and glass, which are often of greater extent than the actual buildings and attract more readily the notice of the public. So that in these examples, whilst we see the architect and the engineer each venturing into the other's domain, or what is usually so considered, we see also that the architect is the chief aggressor and gets most of the spoil. When, however, the engineer reaches the open country, or even the streets of the town, he works his own will on the bridges, viaducts, embankments, tunnels, ventilating shafts, *et hoc genus omne*. Especially with regard to goods

stations and warehouses, the engineer has it all his own way, and directs the expenditure of vast sums on these buildings, acting in the capacity of an ordinary architect. In this respect the architect may consider that his preserves are being poached by the engineer. Turning now to another branch of engineering, that connected with waterworks, what do we find? The reservoirs and pipe lines, and the works connected therewith; tunnels, bridges, and pumping stations, with their buildings and machinery, all come naturally within the engineer's legitimate business. And none of these works are now even thought of as belonging to architecture, although there is much scope for architectural taste in many of the embankments, aqueducts, towers, engine-houses and such like structures. At any rate, since the rise of engineering as a separate profession, the architect has had to yield up possession of these works. When, however, the water has been duly conveyed to a large town, and the question of providing public baths and wash-houses presents itself, the architect either steps in or is called in, and the buildings, at least, are made to receive the impress of his art, even though the actual purposes of the edifice have to be fulfilled by the special work of mechanical and hydraulic engineers. In some cases, it is true, the local authorities do not employ an independent architect to design such buildings, but entrust them to their own town surveyor, borough engineer, or water engineer, or whatever his official style happens to be. This officer, from the nature of his duties, has really a dual character. With regard to the now more or less distinct vocations of engineering and architecture, he has to fulfill a double function, which is, of course, not confined to the erection of the buildings we have mentioned, but extends to all the various engineering and architectural works of the public authority in whose service he is engaged. And so with the dock and harbor engineer. Although in his case, undoubtedly the bulk of his work is such as must be classified now-a-days as engineering, yet he is called upon to design and construct many buildings and other structures which have, or should have a decidedly architectural character, such as piers, lighthouses, hydraulic machinery buildings, public waiting-rooms and offices, clock towers, and other erections. Here, again, the engineer may be said to trench upon the hereditary domains of his cousin the architect. Even in connection with the partially lapsed art of canal making, the engineer is probably destined to renew his acquaintance with the architectural features of numerous locks, bridges, and aqueducts, to be constructed (even in this iron age) for the most part of stone, which has always been the pet material of the architect. As to the followers of the more purely mechanical branches of engineering, so closely connected with machinery, they are developing a kind of natural affinity for architectural work in quarters where it was least expected. In times not long past, the projectors of extensive factories and works to be fitted up with peculiar or costly machinery were accustomed to employ an architect for the erection of the building and a special engineer for the supplying and fixing of the machinery. Considering that an ordinary architect is equally ready to design a church or a distillery, it is hardly surprising that his Ishmaelite relation, the engineer, should wrest from him some of the specialties, and appropriate them to himself. Accordingly, we find new tribes of the great engineering family flourishing as gas works engineers, sugar works engineers, brewery engineers, cotton and silk mill engineers, and so forth, who undertake the designing and constructing of the great piles of buildings which are to enshrine the machines and engines required for that particular trade or industry. Occasionally it happens, nevertheless, that an architect of high standing, chiefly concerned with the more artistic side of his vocation, is employed to plan and execute buildings which are now by general consent, regarded as the proper work of the engineer. In such cases as these, whether it is of their own will or at the will of the public, the members of the two professions are playing a friendly game of tit for tat. The present condition of things, then, appears to be this: The practical relation between engineering and architecture are not sharply defined nor carefully observed. We may now recur to the question whether those relations ought to be modified. If so, should the two great branches of constructive

skill be drawn closer together, or should they be made more distinctly separate? Now, can it be maintained that the present state of affairs is satisfactory? This is not a quasi-philosophical question, but a very practical one. Two kinds of interests are involved in it—the interests of the persons whose occupation or livelihood is concerned in it, and the interests of art in its æsthetic aspect, whereby intellectual happiness is influenced. Well, in so far as uncertainty and confusion exist in the relations between engineering and architecture, it seems expedient that their boundaries should be more exactly laid down. Like two great political States, these two great professions, as they grow more powerful and approach more closely, have the greater need of a clear understanding as to their natural and scientific frontiers. In this age, few professional men can govern in both provinces. Even the admirable Crichtons will have enough to do with their talents in either domain. But as things are, we see one practitioner styling himself "Civil Engineer and Architect," whilst another is described as "Architect and Civil Engineer." These men are no doubt at present performing a special and useful function. But the race will die out. A pupil articled to such an engineering architect must be greatly perplexed by his divided allegiance to Rankine on the one side, and Palladio or Pugin on the other. It was much easier for Tintoretto to live up to his motto, "The day to Titian; the night to Michael Angelo," than for a nineteenth-century student to set his affections profitably on Gothic vaulting or the Ionic capital, when he is chiefly engaged in a sewerage scheme, or a system of tramways. Clearly, it is desirable that he should understand, as far as possible, the distinction between engineering and architecture, even if he has to draw an arbitrary line for his own observance. But in thus making the two professions more distinctively separate, there is no reason why the natural bond between them should not be respected or even drawn tighter. If architecture is the mother, and engineering the daughter, they should be on good terms. Nevertheless, a man does not marry his mother-in-law, and, as a rule, they agree better when living apart. Let the engineer and the architect each stick to his last. Whilst, however, he practices his special calling only, he ought to have a considerable knowledge of the other profession in those points where he necessarily touches it. The architect cannot be well qualified generally if he is ignorant as to the capabilities of iron columns and girders, and of concrete floors, the overturning force of the wind, the pressure of embankments against walls, and the laws of mechanics. Nor can the engineer satisfactorily design his bridges and towers if he has no knowledge whatever of either the Classic orders or Gothic styles. For, although, as Fergusson remarks, "it is not essential that the engineer should know anything of architecture, it is certainly desirable that he should do so. On the other hand, it is indispensably necessary that the architect should understand construction. Without that knowledge, he cannot design; but it would be well if, in most instances, he could delegate the mechanical part of his task to the engineer, and so restrict himself entirely to the artistic arrangement and ornamentation of his design. This division of labor is essential to success, and was always practiced where art was a reality; and no great work should be undertaken without the union of the two. Perfect artistic and perfect mechanical skill can hardly be found combined in one person, but it is only by their joint assistance that a great work of architecture can be produced." If this be so, and it will hardly be doubted, the work of the man who styles himself architect and engineer is not likely to be of the very highest merit. In the present relations between the two professions, however, such a practitioner makes himself respected or feared on both sides, and deservedly so. But as the distinction between them becomes better defined and more generally recognized by the public, his position will be increasingly difficult, and in the end untenable. This need cause no regret, for, as we have seen, it is desirable in the interests of both professions that they should be as much as possible *practiced* apart, even when a considerable acquaintance with both confers as advantage on its possessor. We appear, then, to have been led, whether we are willing or not, to the conclusion that engineering and

architecture ought to be made more distinctively separate. But can they not, at the same time, be in some way more closely united? If an ordinary man is not Colossus enough to bestride the strait between the two professions, may he not take his stand on one side and join hands with his friend on the other? Now this thought brings us to a practical suggestion, with which this paper may fitly be drawn to a close. Seeing that engineering and architecture are both concerned with building work, and must always approach each other more or less nearly, it would probably be a successful working arrangement in many cases if a well-qualified engineer and a well-qualified architect were to join in partnership. Such a style as "Septimus Jones, F. R. I. B. A., and Orlando Smith, M. Inst. C. E., Architects and Engineers," if justified by the quality of the work done by the firm, would carry weight with the public, and would secure many commissions which Jones or Smith by himself would fail to get, or would imperfectly carry out. Thus we finish with a marriage, and everyone is happy, or ought to be so. Should the blessing of Providence rest on it, and any good issue result from it, then the time spent in considering this subject will not have been utterly wasted.

THREE PROBLEMS IN RIVER PHYSICS.

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From the Proceedings of the American Association for the Advancement of Science.

In the past five or six years, there has been a great deal of money spent upon, and study given to, many questions of a scientific nature pertaining to the improvement of our western rivers. Your attention is called to some conclusions that may be drawn from recent investigations on the three following problems:

I. The Transportation of Sediment and the Formation and Removal of Sand Bars.

II. The Flow of Water in Natural Channels.

III. The Relation of Levees to Great Floods, and to the Low Water Navigation of Rivers.

I.—*The Transportation of Sediment and the Formation and Removal of Sand Bars.*

The solid matter carried by streams of variable discharge and cross-section, flowing over sandy beds, is at once the cause of, and remedy for, most of the obstructions to the navigation of such streams. A proper understanding of this subject is a prime requisite to an adequate conception of the conditions governing the flow of water in natural channels, the improvement of low water navigation, flood confinement, etc.

In all natural water courses, the material carried by the stream may be graded according to the method of its transportation, as,

First—That carried in continuous suspension.

Second—That carried in discontinuous suspension.

Third—That carried by rolling on the bottom.

Sediment in Continuous Suspension.—This is composed of such finely divided particles of clay and mud that any slight disturbance of the water in a vertical direction is sufficient to prevent its deposition, and so it mostly remains in suspension until it reaches the sea. A large part of the sediment carried by the Mississippi River is of this class. It mostly comes from the western tributaries, more especially from the Missouri. If a quart of this water be placed in a glass jar, and set away, it requires some ten days for the water to become clear. Evidently, this kind of sediment does not form sand bars, neither can the engineer avail himself of it in building artificial banks in contraction works. It is therefore of no consequence to the river engineer. It is neither helpful nor harmful, and may be ignored.

Sediment in Discontinuous Suspension.—This kind of sediment is composed of sand, more or less fine, according to the velocity of the stream. It is the material of which sand bars are made, and it is also the material out of which the engineer builds his artificial embankments by the square mile behind his permeable dykes. It is constantly being picked up at one point and put down in another by the action of a law as unchangeable as the law of gravity. In the river's tireless efforts to attain a uniform flow, it is constantly scouring out the engorged sections, and filling in the enlarged sections. For any given stage of water, the volume discharged at successive sections is the same, but since the sections do not have equivalent areas the mean velocity at successive points is a constantly varying quantity. The river undertakes to overcome this inequality and so engages in an enormous carrying trade, which consists in cutting out the bottom where the section is less and the velocity more, and carrying this sand to the first point below where the section is more and the velocity less, and there it is deposited. Evidently, if any given stage of water should continue long enough, this double action of scouring the small sections and filling the large ones would result in a nearly uniform flow, when such small particles as could be carried by the resulting velocity would be so carried in permanent suspension, leaving a bed of tolerably permanent character, being only influenced by the matter rolling on the bottom. But here comes the trouble. No sooner has the river set to work to adjust itself to any given stage, than behold, the stage changes, and then it must go to work, cutting and filling at new points, and as the stage is ever changing, the river is ever beginning anew in its endless task.

It may not be clear why this change in stage should so disarrange matters. Let us see. The cause is the great variation in width. Take an actual case. At Plumb Point, on the Mississippi River, sixty miles above Memphis, the following extremes were found in 1879 on a reach eight miles in length.*

		Wide Section.	Narrow Section.
Surface width, in feet,	High water.	8840	1680
	Low "	7330	1375
Mean depth, in feet,	High water.	23.9	60.7
	Low "	6.0	23.9
Area of section in sq. ft.	High water.	260000	94500
	Low "	30220	68500
Mean velocity in ft. per sec.	High water.	3.67	10.1
	Low "	2.65	1.15
Fall per mile in feet,	High water.	0.26	1.75†
	Low "	1.15	0.07†

Thus, at low water, the wide section had a mean velocity of 2.65 feet per second, while the narrow section had a mean velocity of only 1.15 feet per second. At a stage 26.6 feet above low water (which in the table here given is called "high water," although it is some eight feet below extreme high water) the conditions are reversed, for now the wide section has a mean velocity of 3.67 feet per second, while the narrow section has a mean velocity of 10.1 feet per second. Here the wide section was some eight miles above the narrow one. Evidently the river is cutting out the shoal and filling up the pool in low water, and cutting out the pool and filling on the succeeding shoal in high water, and in fact this is what is always observed to occur.

The amount of this local scour and fill on the Mississippi river is enormous. In the case above noted there was a total scour over the reach of 350,000,000 cu. ft. or enough to cover one square mile to a depth of 11 feet. This occurred from Nov. 13 to Jan. 3. In many cases the rate of fill and scour, as well as the total amount, is much greater than this. It is not uncommon for the shoal places to be built up as much as from six to ten feet in time of high water, so that the bottom of the river is then higher than the surface of the water will be after this same fill has been cut out by the succeeding low water. In other words, the river bed is a succession of narrow, deep pools, alternating with wide shoals. The pools are in the bends, and the shoals are on

* See Report of Miss. Riv. Commission for 1881, p. 69.

† From plate 7, p. 120 of same report.

the crossings. The shoals are like so many dams, fifty to seventy-five feet in height, stretching across the river bottom say every ten miles. In high water these wide dams are built several feet higher, and the narrow pools are dug several feet deeper. In time of low water the dams are scoured down several feet, and the material deposited in the succeeding pool. What is true on the Mississippi river on a large scale, is true of every stream flowing in a friable bed, on a corresponding scale. This sediment is, therefore, said to be in temporary or discontinuous suspension. It settles very quickly where a sample is caught in a vessel, and the amount carried by the river is only determined by taking samples at various depths. There is little of it near the surface, except when there is violent vertical agitation, as in "boils" and eddies. It is constantly and rapidly falling towards the bottom, and is only prevented from reaching it by the upward motion of the medium.

Material Transported by Rolling on the Bottom.—When the grains of sand are too large to be held in temporary suspension they may yet be moved along on the bottom from being unable to resist the force of the current. This motion is also mostly discontinuous inasmuch as the particle is pushed up the gentle slope of the upper side of a sand wave or reef, and dropped upon the abrupt down-stream side, there to remain until the reef has progressed so far as to again expose it to the action of the water on the up-stream side. The amount of material so transported is fairly represented by the size and rate of travel of the wave.* These sand waves, or reefs, may be as much as 8 to 15 feet high, extend from one-half to three-fourths the distance across the river, and move from 10 to 30 feet a day. They succeed each other at intervals of some 300 feet. The same action is seen by the roadside, on a small scale, after a shower. It obtains in all natural water courses, is found in both pool and shoal alike, but is most strongly developed on the shoals. It is largely instrumental in the formation and removal of sand bars, but is of little use to the engineer in building embankments. It is in this way, however, that the slow currents of a low stage may cut a channel through a wide and high bar. Although the mean velocity may be small, the bottom velocity is relatively large on account of the shallow depth. The period of its action is also apt to be longer than that of flood stages. The amount of matter so transported is, however, small on the Mississippi river, compared to that carried in suspension. A knowledge of this action is mainly important as going to explain the regular changes that are observed to take place on the bottoms of cross-sections of rivers.*

II.—*The Flow of Water in Natural Channels.*

It is now generally admitted that no formula, involving only the variable functions of slope and cross-section, can ever be used to give even approximate values of the mean velocity, across a given section, of a stream flowing in its natural or irregular channel. Farther than this, we are now forced to the conclusion, that no such formula can ever be used to give the continuous discharge of a single stream, at a given station, even though the actual observed mean velocities on that section, for one year, be used in its derivation.

The common type of formula is

$$v = c\, r^m s^n$$

where

v = mean velocity across the section.

c = a constant coefficient, either general or specially derived for each case.

r = hydraulic mean depth $= \frac{\text{area of section}}{\text{wetted perimeter}}$.

s = slope = sin. of angle of inclination to the horizontal.

m and n are usually taken as $\frac{1}{2}$.

The Chezy formula, which is of this type is:

$$v = c\sqrt{rs}.$$

Most of the efforts made to adapt this formula to the flow of water in natural channels have been spent upon the coefficient c.

It is, however, no longer a question of coefficients or exponents. For regular

* See Report of Chief of Engrs. U. S. A. for 1879, Vol. III., p. 1963: also, Reports of Miss. Riv. Com. for 1881 and 1882.

* For an article on "Silt Movement by the Mississippi," by Robt. E. McMath, see "Journal of the Association of Engineering Societies," v. 1, p. 266.

and stable channels, like flumes, canals, or ponds controlled by fixed weirs, formulæ can readily be deduced that will give fair results, but in all natural channels, the cross-section, slope, and flow, are so irregular, and the bed so changeable, that the variables affecting the flow are not adequately represented by r and s, and hence, any formula based on these for the independent variables will surely fail.

A body of finite mass, moving under constantly varying accelerating and retarding forces, never has a velocity at a given point which is a function only of the forces acting at that point. Since the accelerating forces of a stream are due to the slope, and the retarding forces are due mainly to bed and banks, and since the conditions of slope, bed and banks are constantly changing in the progress of a given mass of water down stream, it follows that this mass is subject to constantly varying forces. The velocity of this mass at any section is therefore a function of the slope and bed for an indefinite distance above, and can never be predicted from the immediate conditions at the section.* Whether or not it can ever be predicted, within reasonable limits, is still a question, but some new light has recently been shed on this much mooted subject. If the conditions of slope, approach, cross-section and discharge, should be always the same at the same stage of water, or for the same stage and same rate of rise or fall, then a cycle of observations on mean velocity might enable us to predict what the next cycle of mean velocities would be at that same section. But, as outlined above, in discussing silt movement, it is seen that, in streams of unstable *regime*, the bed is constantly shifting, and that the high ridges, or bars, that alternate with the pools, are constantly changing their height. If these ridges be conceived as weirs, and the stage of water in the intercepted pools be controlled by these weirs, which it is in medium and low stages, then the discharge in this pool may be discussed with some confidence and profit. If the section be located in the lower portion of the pool, so that the conditions of approach over the weir above be of little consequence, then the conditions effecting discharge are, the *effective head*, that is, the surface slope from the section to the weir below, and *the depth on the weir*. The origin, from which the stage should be measured, in this case, is the natural zero of hydraulic activity, viz.: the horizontal plane passing through the crest of the weir. When so taken, a very simple relation is found to exist between *stage* and *mean velocity*, inasmuch as the latter is a linear function of the former.* If this relation be determined by observations at that section, then it may safely be used *so long as the weir condition remains fixed.* When this changes, by scour or fill of the bar, then the origin of the locus changes with the change in origin for stage. But the weir condition could be made the subject of observations, and the locus of the curve of mean velocity and stage so adjusted as still to indicate the mean velocity for this locality, provided the section on weir or bar should remain, the section controlling or limiting the discharge. In high stages, the region over the weir becomes itself the comparatively stagnant pool, and the controlling or limiting section is in the deep and narrow bend below. The low slope is now over the shoal, and the steep slope in the deep pool. This slope is used to generate the accelerated velocity through the engorged section. The condition of a slack pool, retained by a submerged horizontal weir, now no longer obtains, and the narrow, deep reach around the bend with high slope, is similar to an engorgement by lateral contraction. This is a very different problem, evidently, from what we had at a lower stage when the weir section was the limiting one, and a discussion of this state of affairs offers greater difficulties than the other. As a striking example of this change of location of the engorged sections, see the table given above in discussing the movement of sediment. Thus, in low water, the deep section had an area of two or three times that of the shoal section, and a slope of one-sixteenth as great, while

* The problem is similar to the instantaneous relation existing between the pressure on the piston of a crank engine, the resistance to motion, and the velocity of the fly wheel. No one would for a moment suppose that a formula could be derived that would express this relation for any given instant, and yet this is exactly what has been attempted on streams for the last one hundred years.

* The locus is probably an hyperbola, which becomes practically a straight line for all stages above a few feet. See paper by R. E. McMath before Am. Soc. of Civil Engineers. V. 11, No. 239.

at high stage, the deep section had an area of only thirty-six hundredths of that of the shoal section, but its slope was seven times as great.

The object of this presentation is not to discuss or derive formulæ, but to call attention to some of the heretofore neglected functions of the problem. These functions are seen to be so various, and apparently lawless in their nature, that it is highly improbable that we shall ever be able to obtain any formula of much value. A service has been rendered, however, when the impossibility of a successful solution on the lines of investigation so long pursued has been pointed out. Also, when the real determining causes effecting flow are properly conceived and investigated, rational and valuable formulæ may be obtained for exceptional cases of stable conditions.

III.—*The Relation of Levees to Great Floods and to the Low-water Navigation of Rivers, as Illustrated by the Mississippi River.*

That levees may be built which will confine the greatest flood on the Mississippi river, without danger of their giving way, may be admitted. The question is, What shall be their size and location?

The location of a levee is determined by two considerations in favor of putting it as near the bank as possible, and by two others in favor of putting it as far away as possible. The arguments in favor of putting the levee near the bank are:

First—The land near the bank is generally higher than anywhere else. There is generally a well defined slope away from the river, and therefore, a levee here, to confine a given flood plane, will have the minimum height and cost.

Second—The land near the river bank is always most valuable for cultivation on account of its being higher, and therefore the protection of this narrow strip is the chief cause for leveeing the river at all.

The arguments in favor of putting the levee at a distance from the river are:

First—So that it will not fall into the river from craving banks, and *Second*, so that the facilities for flood discharge may be greater, and therefore, the flood stage and necessary height of levee less. This latter argument is so generally advanced that it is deserving of mention, but I will try to show that it is more visionary than real. The force of the other arguments above stated is evident, and they need no further discussion.

The height required for levees in a certain locality is a very complicated problem, and unfortunately for the lower Mississippi river, it is a problem on which we have no direct evidence. By having no direct evidence, I mean that, as there is no point on the river below Cairo where the whole of a great flood has ever been confined between levees, so there is no argument from experience.

In this dilemma we are forced to fall back upon theoretical considerations.

For the past three years, 1882,'3 and '4, we have had great floods on the Ohio and Lower Mississippi rivers, and a great deal of data has been obtained, such as we have never before possessed. The first installment of this flood data may be found in the Report of the Mississippi River Commission for 1883.*

The overflow water of a great flood loses itself out over the west bank of the river from Cairo to Memphis, passes down through the St. Francis swamp, and is forced back to the river again by the high bluffs at Helena. It then overflows the east bank, passes down through the Yazoo bottoms, and is again forced back to the river by the bluffs at Vicksburg. It then once more crosses the channel and escapes over the west bank to return no more, but to find its way to the gulf through the Tensas and Atchafalaya river regions.†

Notwithstanding the river has been leveed almost from Cairo to the Gulf, and in the lower parts of the valley these levees have been carefully maintained and gradually enlarged, yet in time of a great flood the surplus water seems quite oblivious of such frail barriers, and always goes over and through them very much as though it were quite unconcious of their presence. And every time the levees are destroyed the people seem as much astonished as though that were the first time such a calamity had ever befallen them. The trouble is that due weight

*Supplemented in the Report of 1884, by several reports on the observed volume of the overflow water in the flood of 1882, including one by the writer on the Flood Discharge through the Yazoo Bottoms.

†See paper by the writer on "Great Floods on the Lower Mississippi," in Journal of the Association of Engineering Societies, v. 2, p. 115.

has not heretofore been given to the enormous quantity of water passing outside the channel. Before the flood of 1882 no even approximate determination of the amount of such water had ever been made, and no one suspected it to be so large. From the observations taken on that flood, which was the largest on record, we find that on some zones, or belts, there was about as much water passing outside the channel as there was in the channel. The total maximum discharged in that flood was some 2,000,000 cu. ft. per second, while for a hundred miles (by river) at and above Lake Providence, and for another hundred in the vicinity of New Orleans, the discharge in the channel, with overflowing banks, was but 1,000,000 cu. ft. per second. The other million was finding its way to the gulf through the swamps. This is a very startling fact. It means that if we propose to completely confine the waters of a large flood between levees, we must, in places, carry twice as much water as the present channel can take. At all points below Cairo, it is now pretty well known how much more water would have passed in a confined channel, than did so pass in the flood of 1882. With such facts as a basis for estimates, engineers still differ very widely as to the necessary height the levees must have to confine these additional volumes. Some engineers affirm that the levees need be but little, if any, higher than they now are, while others insist that in places they will have to be from ten to twelve feet higher than they now are. The whole question turns on whether or not the bed of the river will scour out when all the water is confined to the channel. The popular conception, as well as the expressed opinion of most writers on the subject, is to the effect that a confining of the waters to one channel will increase the velocity of the stream (which everybody admits), that this increased velocity will give increased scour (which everybody admits, in a sense) and that this increased scour will result in a general lowering of the bed (which must be denied).

In other words, there are two schools of engineers, the concentration school, and the equalization school. The former affirms the beneficial results of an increased volume at high stage; the latter not only denies any beneficial results, but affirms that actual harm would ensue. Again, the question turns on the methods of transporting sediment. The concentration school argue as though all sediment were of the continuous type, and when once taken up it would be carried to the sea, provided all the water remained in the channel. But they say, in effect, that in case of a flow over the bank, while the water pauses to consider whether it shall go over the bank or remain in the channel, the sediment escapes from its grasp and settles to the bottom. On the contrary, we know, that when two forces, at right angles to each other, act on a moving particle, it moves over a path which is a resultant of the two forces, and so moves faster than if either force acted alone. Therefore, instead of the water being checked by the escape over the bank it is really accelerated. Again, the more the energy of a river is increased by increasing the stage the more unequal becomes the difference in area of successive sections in wide and narrow places, and at high stage the narrow and deep places are always the engorged sections. If the energy of the river be increased at this stage it simply scours out the deep places deeper, and drops this matter on the succeeding wide shoal. That is, any influence toward a temporary adjustment of the river's bed to a high water discharge is an influence which is adding to the heights of all the sand bars on the river. The greater the stage, the higher the bars and the deeper the pools become; and so far is this from facilitating the discharge, that, in the language of the Secretary of the Mississippi River Commission, "the effect of an approaching flood is to impede its own discharge, and the impediment outlasts the flood." An example of this action is found in the flood of 1858, at Columbus, Ky., where the river was leveed. The discharge of the river exceeded 1,100,000 cu. ft. per second, four times from December to June. The stages at which the river discharged this amount were: 29.5 ft., 32.5 ft., 33.5 ft., and 35.0 ft. on a rising stage, and 31.6 ft., 34.7 ft., 35.9 ft., and 36.9 ft. on a falling stage.

If the successive differences between rising and falling stages at which the discharge was 1,100,000 cu. ft., be taken out for each rise, we have 2.1 ft., 2.2. ft., 2.4 ft. and 1.9 ft., as the legitimate effect

of the change from a rising to a falling condition.

If, however, we compare the first and last rising stages when the discharge was 1,100,000 cu. ft., we find that in June it required a stage 5.5 feet higher to discharge this water than it did the previous December. Making the same comparison on falling stage we find a difference of 5.3 ft. This was due to deterioration of channel caused by a building up of the bars or weirs, by a sufficient amount to raise the stage 5.5 ft. in order to discharge the same amount.

It is highly probable that the two high waters of 1882 and 1883, in the Ohio River worked such detriment to the channel that the flood of 1884 was raised several feet higher than it would have been had the same amount of water passed in 1882.

It would seem, therefore, that in the river's present condition, there is no evidence that a confined flood will scour out its bed so as to facilitate the discharge, and there is considerable evidence against it. If the river flowed between straight, parallel bank, such as Capt. Eads has constructed at the mouth of the river, then there could be no such thing as discontinuous transportation of sediment, and hence no alternate scour and fill. Then concentration of volume would be beneficial, and would ultimately lower the river bed. But this condition of things can never be reached on the Mississippi River, and hence the concentration of flood volume will ever be harmful rather than helpful.

It follows from this, that if an additional quantity of water is confined to the channel in time of flood, we must provide accommodation for it at the surface rather than at the bottom of the river. If this point be settled, it is not difficult to approximate to the new stage to pass the new volume. There are many stations on the river below Cairo where discharge observations have been conducted for one or more years. If, for one of these sections, we plot stage and discharge, we get a curve showing the relation of the two from low water to say 50 feet above. If the levees be near the banks, which they usually are, and they be high enough to confine the flood, the condition of flow will not be materially changed as the water gets above the bank. If, now, we know what the additional quantity will be to confine it all, we could *simply extend the discharge curve* until we reach the proper volume, and then read off the corresponding stage. The only uncertainties here are in the curve itself, which may not rigidly apply for another year at the same locality, and in its extension. These uncertainties may have a maximum value of several feet, and yet it is the only argument we can use, and it will give approximate results, which are more likely to be too small than too large. If the heights of levee necessary to confine such a flood as that of 1882 be found in this way, and the additional cost estimated, it makes an item of over $50,000,000, to complete the levee system from Cairo to the Gulf. It is questionable if such levees ever will be built.*

In regard to putting the levees farther from the banks, the two main objections have already been mentioned. There would be also little advantage from increased facility of discharge. The ground would have to be kept clear of all timber, weeds, and undergrowth, or else it would offer very poor opportunities for discharge. This could hardly be done. The water flowing across a neck, also, does more to hinder than to help the flow around the bend, by lessening the volume in the channel.

There is another expedient which has been advocated, which appears to be practicable, but for which there are yet not sufficient surveys to determine. It consists in having ordinary sized levees, such as are now built, which will confine the ordinary floods, with waste weirs at such points as offer the best facilities of discharge through the swamps, to let off the surplus waters without detriment to the levees and with the least possible damage to the lands. There are a great many natural outlets from the river back to the main drainage channels of the bottoms, which could be utilized in this way. This sort of outlet, however, is more like the spilling of the water over the bank, than like a crevasse in the levee. These weirs would have to be miles in extent so that the depth of overflow need not be more

*The Miss. Riv. Com., in their Report for 1883, estimate this cost at some $11,000,000, but Gen. C. B. Comstock, President of the Commission dissents from this view.

than two feet. This arrangement has been adopted on the Rhone with very satisfactory results.*

The relation of levees to low water navigation has already been trenched upon above in showing the effect of increased stages at time of high water, in building up the bars. What is wanted is the removal of the bars, and for this purpose the low water energy should be increased and the high water effect diminished. In other words, the extremes should be brought nearer together, which is all included in the term equalization of volume. Any influence, therefore, which forces these extremes farther apart is hurtful. Levees tend to make high water higher, and therefore, they are an injury to navigation.*

Evidently it would be of no advantage to navigation if the river bed were uniformly lowered a hundred feet, provided the same irregularities remained in the matter of pool and shoal.

If ever the river should be regulated in width so that the discontinuous movement of sediment becomes inappreciable, then the stage may be increased to advantage. It is highly improbable that this ever will be done, and therefore, the building of levees on the banks of the Mississippi River will never prove an aid to low water navigation. On the contrary, they will always tend to produce a certain amount of damage.

* See paper by the writer on "Protection of Lower Mississippi Valley from overflow" in Journal of the Association of Engineering Societies, v. 3, p. 169.

* See paper by Robt. E. McMath on "Levees,—their Relation to River Physics," in Journal of the Association of Engineering Societies, v. 3, p. 43.

A COMPARISON OF BRITISH AND METRIC MEASURES FOR ENGINEERING PURPOSES.

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I.

Some of the existing systems of measures, such as those of time, angles, velocity and electricity, are practically international. This paper may therefore be limited to comparing the advantages for engineering purposes of those measures of length, surface, capacity, weight and pecuniary value, adopted in England, with the metrical and decimal systems of the same classes of measures.

The system of arithmetical notation used by all civilized nations is decimal; consequently all decimal systems of measures have the advantage over others of dispensing with the compound rules of arithmetic, and of facilitating the use of logarithms in engineering calculations. Among decimal systems of measures, the metric system has the advantage of being already used by 240,000,000 of people, with whom 60 per cent. of the foreign trade of England is carried on, and of all its terms being correlated and systematically derived from one convenient, well known, standard measure of length. The natural character of the metric unit, and the accuracy, or otherwise of its relation to the meridian is practically immaterial. The collection of measures used in England for engineering purposes is partly duodecimal, partly decimal, chiefly binary, and generally unsystematical. Some British measures have the advantage over all decimal measures of being capable of finite ternary subdivision, but this advantage is of comparatively little practical importance.

England and the United States of America hold at present such a prominent position in the manufacture of engineering machinery and material constructed to British scales of measurement, that a considerable practical advantage is on that account derived from the use of British measures in engineering works. The capital invested in machinery constructed to manufacture to sizes of British measurement is very large, and the literature on engineering subjects expressed in terms of British measurement is most

extensive and valuable. English and American engineers have their minds saturated with guiding or standard dimensions expressed in British measures and ready for application; so that the substitution of other measures in their general practice would prove a source of considerable personal inconvenience to the current generation of engineers in England and the United States. Most British measures in common use have the great advantage of being already familiar to the masses of the English and American people, and to the large number of skilled workmen which these people include.

As regards the depreciation in value of English engineering literature, written in terms of British measurement, that would ensue on the introduction of metric measures, it may be remarked that, long before the existing generation of English and American engineers passed away, the gist of the information contained in English engineering books would be translated into the international language of measurement. The question of the depreciation in value of costly machinery, constructed for manufacturing articles to scales and exact sizes of British measures, is more difficult, and the loss and inconvenience from such depreciation must form a considerable set-off against the advantages to be derived from a general adoption of the metric system. It must be remembered, however, that the progress of improvement in machinery, and the wear and tear of existing machines are so rapid, that in a comparatively few years most of the existing individual machines of this kind would either be worn out, or have become obsolete, from other causes than a demand for articles manufactured only to regular metric sizes.

Both the meter and the British standard yard have the advantage of being the approximate length of a human stride, which has always been the readiest and most extensively used natural unit of linear measure for engineering field-work. The length of a meter is about the limit to which a human being can conveniently place his hands apart when measuring with accuracy on a vertical surface.

It may be observed, incidentally, that the British foot is, strictly speaking, not a British standard measure, but is simply a ternary subdivision of the standard yard, and is itself divided duodecimally into inches, while the inches are again divided binarily and fractionally into quarters, eighths and sixteenths. In much actual practice the foot is not even adopted as a unit of linear measurement; for instance, most carpenters use a two-foot rule, and having found the number of lengths of their two-foot rule contained within the length of the plank they are measuring, they proceed to compute this length in feet by doubling the number of two-foot-rule lengths they have measured, and then adding to this the odd foot and inches and fractions of inches which may go to complete the total length of the plank. A workman measuring with a meter simply measures the number of meters, and reads off at once the decimal parts of a meter which make up the total length of whatever he is measuring. It may be said that the amount of computation required to double the number of two-foot-rule length is so small as not to be worth consideration, and that such an objection to measurement by feet is merely fanciful; but for all that it cannot be denied that an additional mental process has to be exerted by a British workman every time he measures a length with a rule which does not coincide in length with the unit in which he expresses the total length measured. A plank can be measured with accuracy with a meter in two-thirds of the time required to measure it with a two-foot rule, and in one-third of the time required with a one-foot rule; and in a carpenter's day's work the sum of these differences of time is by no means unimportant.

The British inch is much too large to serve as the lowest integral unit in a scale of linear measurement, and even the sixteenth of an inch is too large for minute work. The instruction so often given to a British workman to make an article "five-sixteenths full" or "bare three-eighths" are hardly compatible with precise accuracy of workmanship. The millimeter is, on the other hand, a very convenient unit for ordinary minute work, and its decimal subdivisions are quite as convenient for microscopic work as thousandths of an inch. Another example of the additional computation involved by English practice is the taking

of levels for building work in feet and tenths and hundredths, and the necessary translation of the resulting dimensions into feet, inches, and fractions of inches for the workmen.

For all ordinary field-leveling, the division of a leveling-staff into decimeters and centimeters only, is sufficiently minute. The centimeter can easily be binarily subdivided by ocular estimation, and the practical advantage of reading to the hundredth of a foot on a leveling-staff in straightforward leveling is very doubtful. Certainly more rapid, and probably quite as accurate, leveling can be done in a day with a staff subdivided in centimeters as with a staff divided into hundredths of a foot, and the practical accuracy of metric leveling when applied to building work will be greater, as the dimensions given to the workman will be in the terms actually noted through the level.

The chain used in the metric system for field work is usually 20 meters long, or 4½ inches shorter than Gunter's chain, though for difficult country a 10-meter chain is preferable. Assuming that the length of Gunter's chain is that found most convenient for field-work, the twenty-meter chain has practically the same advantages in that respect, but the use of a twenty-meter chain involves additional computation, owing to the number of arrows held by the rear chain-man having to be doubled to get the number of decameters to be entered in the field book. The use of a ten-meter chain of course does away with this objection, and it has the same advantages as regards direct correlation with the hectare as Gunter's chain has with the acre. The twenty-meter chain has the advantage over Gunter's chain that it is capable of subdivision into the smaller linear measures, whence Gunter's link is the lowest integral subdivision of Gunter's chain, and is too large for any work but land measuring.

For purposes of engineering calculation, it is obviously desirable that the unit of linear measure should have its multiples and submultiples arranged in accordance with the universal system of arithmetical notation, that it should be the basis of the units of measure of surface, capacity, and weight, and that it should be as international as possible. The meter complies much more fully with these conditions than the yard. In all questions relating to the pressure of materials upon surfaces, the intimate correlation existing in the metric system between the units of measures of weight and capacity is valuable, and hydraulic calculations are much simplified thereby. Between the English horse-power of 550 foot-pounds per second and the metric horse-power of 75 kilogrammeters per second there is little practical difference in point of convenience, but it is highly desirable that scientific units of this nature should be international.

An engineer using the metric system has practically but one denomination of measurement to deal with throughout his work, both in the field and in the office; all his measurements and the scales of all his plans being expressed decimally in terms of a meter. An engineer using British measurement measures a line of works in chains, furlongs, and miles; takes levels in feet divided decimally; gives dimensions for buildings in feet divided duodecimally into inches and in inches divided fractionally; calculates earthwork in cubic yards; measures brickwork in rods, and land in acres, roods, and poles; measures lime in bushels or barrels, water in gallons, and weighs materials generally in tons, hundredweights, quarters, stones, pounds, and so on, almost *ad infinitum*. Then when he comes into the office he plots his surveys in scales of chains to the inch and feet to the inch, and makes his drawings in scales of various fractions of an inch to the foot.

Besides the measures and scales above specified, the English or American engineer has to deal with an infinite number of gauges, trade sizes, and trade and local customs which render comparison of cost exceedingly difficult, and many of which would become obsolete if the metric system were in general use. The habitual employment of British measures debars English and American engineers to a great extent from profiting by the extensive and valuable foreign technical literature written in terms of metric measurement. However perfect they may be in knowledge of foreign languages, their minds cannot, if they have been accustomed to work only with British measures, either grasp or retain with facility, dimensions expressed in metric measures, and they are therefore partially shut out from a very extensive source of profes-

sional experience. The advantages of an international language of measurements for engineering purposes are so great, that they may be assumed to counterbalance any advantages claimed on behalf of any new decimal systems based on units of British measurement.

In so much of the routine work of a civil engineers' office as consists in taking out quantities and making estimates, about one-third more work can be done within a given time with metric measures than with British measures, though, of course, the loss of time can be somewhat reduced by the free use of ready-reckoners and tables. On this account, engineers accustomed to and using metric measures, have an advantage over engineers using British measures. It has been urged as an objection to decimal systems that they are not suitable for binary subdivisions, and alleged that binary subdivision is the most convenient for common purposes. It must be remembered, however, that as all binary divisions admit of expression in precise decimal equivalents, there is nothing to prevent binary divisions of a metric unit expressed in fractions, such as $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$, &c., being used by those who prefer them. The metric scales for plans are simple and convenient, and have the advantage of enabling the mind to compare very easily the dimensions on plan with the actual distances in nature.

The English practice of plotting plans to scales of so many of Gunter's chains to the inch, expressed in feet, seems to have little to recommend it beyond the questionable advantage of causing the admeasurement of such plans by the uninitiated to be particularly difficult. In a paper "On the Introduction of the Metric System into Machine Shops in America," Mr. Coleman Sellers, M. Inst. C. E., points out that the metrical scales only admit of a descent in integral vulgar fractions from full size to $\frac{1}{2}$, $\frac{1}{5}$, $\frac{1}{10}$, $\frac{1}{20}$, $\frac{1}{50}$, $\frac{1}{100}$, whereas the ordinary inch rule furnishes scales of $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{6}$, $\frac{1}{8}$, $\frac{1}{12}$, $\frac{1}{16}$, $\frac{1}{24}$, $\frac{1}{32}$, $\frac{1}{48}$, $\frac{1}{64}$, making twelve scales in all, as compared with seven metrical scales. Draftsmen, however, are not confined to the use of either an ordinary inch or an ordinary meter rule in drawing-offices; and in some machine shops mechanics are, for good reasons, absolutely prohibited from laying a rule on a working drawing, and instructed to work solely from the figured dimensions.

In the same paper Mr. Sellers states from personal experience that the metric system is not well adaped to the machine shop, because it does not admit of advancing in a series of sixteenths of an inch, which he implies to be the most useful and salable series for certain articles. No doubt, as the "ordinary inch rule," divided into binary fractions of an inch, has been the ordinary linear measure used by purchasers of certain articles for numbers of years, they will prefer such a well-known series of sizes, and they are therefore found at the present time to be the most salable; but there is no reason to suppose that purchasers would continue to prefer a series of sizes advancing by the exact metrical equivalents of sixteenths of an inch if the fractional divisions of an inch were no longer in ordinary use as linear measures.

As regards building scales it seems desirable for simplicity that the scale of $\frac{1}{8}$ inch to 1 foot, or $\frac{1}{96}$, should be replaced by a scale of 1 centimeter to a meter, or $\frac{1}{100}$, and the scale of $\frac{1}{4}$ inch to a foot by a scale of 2 centimeters to a meter, or $\frac{1}{50}$; while such a building scale as $\frac{3}{16}$ inch to a foot can scarcely be held to convey a very clear idea of the relation between the size of the building as represented on paper and its actual size when built.

It has been generally conceded that the maximum advantage from the use of the metric system for ordinary purposes cannot be obtained unless the system is used in conjunction with a decimal system of coinage as measure of pecuniary value; and this applies nearly as much to its use for engineering purposes in the preparation of estimates and in recording the cost of work.

It has been further conceded that measures of pecuniary value differ from other measures, insomuch that for political reasons they can never be expected to become international. The reasons given by the late Sir John Herschel and by Sir George Airy, Hon. M. Inst. C. E., for preferring the decimal subdivision of the pound sterling to any other decimal arrangement of British measures of pecuniary value, appear unanswerable; and if so, the only practical question remaining is the simplest and least unpopular means

of bringing such an arrangement into use. It was long ago suggested that by simply enacting the depreciation in value by 4 per cent. of the existing bronze tokens, and thereby making twenty-five farthings legal change for a sixpenny piece, the British currency would be at once completely decimalized. New coins would not be necessary, although the coinage of a silver or nickel ten-farthing piece might be desirable, and the coinage of an increased number of farthings would be necessary in order to provide the odd farthing that would be required, in addition to any combination of pennies and halfpennies in giving change for a sixpenny piece. The existing pennies and half-pennies would serve as four-mil and two-mil pieces respectively in the terms of decimal currency, while popular sentiment would be appeased by the retention of the familiar coins. The alteration of the national measures of pecuniary value, however, could only be effected by compulsory legislation, whereas the general adoption of the metric system, which is already legal, though not compulsory, might be brought about to a great extent by its advantages being gradually impressed upon the common sense of the masses through the influence and legitimate pressure of the engineering profession. It has been found, moreover, by the experience of the introduction of the metric system on the Continent of Europe, that the difficulty of familiarizing the working classes with the system had been much overrated. In Austro-Hungary, for instance, the metric system was extensively used by engineers for their own convenience for some years previous to its general introduction. It had become the interest of contractors and workmen to make themselves acquainted with the terms of measurement in which the plans, specifications, and bills of quantities supplied to them by their employers were expressed, and thus through the medium of the working class a knowledge of the system had been extensively spread through the mass of the people before the system was made legally compulsory for the purposes of general commerce.

Very little progress has been made in England, or America, within the last twenty years towards simplifying measurement; and the general public in both countries appear perfectly indifferent on the subject, although the actual loss of money, through time wasted on computations and arithmetical studies which the use of the metric system would render unnecessary, must amount to an immense sum annually. Judging from continental experience, it would seem that the best prospect for a reform of measures in England lies in the initiative being taken by those professions most directly interested in the matter.

If British engineers were to commence to take measurements, and make plans and drawings in metric measures, they would probably confer a considerable balance of advantages upon themselves, and conduce to confer great eventual benefit upon their country. Whatever views may be held by individual members of the Institution, as to the advisability or otherwise of using metric measures in their own private practice, the author submits that the council might well consider the advisability of taking steps towards obtaining such amendments of the Standing Orders of the Houses of Lords and Commons, as would permit of the dimensions on parliamentary plans and sections, and measures in parliamentary estimates, being expressed either in metric measurement or in British measurement, at the option of the engineer. If parliamentary plans and estimates could be prepared in metric measurements, English engineers would have practical opportunities of testing the relative advantages of metric and British measures; and such a test would be more useful than any amount of writing and abstract discussion on the subject.

The decimalization of the pound sterling would upset existing arrangements so little that it is hard to see why it should not be enacted at once, and the decimalization of the coinage would conduce greatly to acquainting the masses with the advantages of decimal computation, but compulsory introduction of the metric system by legislation seems hardly desirable until after the coinage has been decimalized.

Discussion.

Sir Frederick Bramwell, President, proposed a vote of thanks to Mr. Hamilton-Smythe. All should thank him—those who agreed with his views for ad-

EXAMPLE.

To find the weight of water in a tank measuring:—

Ft.	Ins.	Ft.	Ins.	Ft.	In.		M.		M.		M.
10	×	6	×	2	× 1	1, or	3.20	×	1.88	×	0.33
				10.5					3.2		
				6.17					1.88		
				735					256		
				105					256		
				630					32		
				64.785					6.016		
				1.083					0.33		
				194355					18048		
				518280					18048		
				.647850					1.98528 tonne.		
				4) 70162.155							
				4) 17540.538							
				4) 4385.134							
				7) 1096.283							
				4) 156.611							
				20) 39.152							
				1.957 ton.							

vocating them, and those who did not, for the opportunity of stating their objections. He would beg the author to be good enough to explain the tabular comparison given in the example to the paper.

Mr. Hamilton-Smythe said he had put forward a mode of calculation, almost at random, with which engineers were familiar. He had taken British measures to find the weight of water in a tank 10 feet 6 inches by 6 feet 2 inches by 1 foot 1 inch, and also the nearest equivalent metric measures—3.20 meters by 1.88 meter by 0.33 meter. He had done this in British measures approximately. He had not converted the cubic feet into the exact number of lbs., but had assumed 1,000 oz. to the cubic foot, the more exact weight being 998.2 oz., or thereabouts. Taking 1,000 oz., produced an error, but shortened the process. To effect the calculation with British measures by the shortest way required ninety-four figures. It was done accurately in metric measures with thirty-five figures.

Mr. Henry Maudslay said the author had not done himself or his subject justice in putting that calculation in such a form, because British measure of feet and inches would have to be reduced first of all to the lowest denomination before calculations were begun, or else it would be necessary to have a book of decimals, or to work it out in some other form, before arriving at the results which had been obtained. The "5" in 10.5 meant "6" inches. There was thus a mental calculation as to the half of a foot put in decimal terms, and to put measurements of feet and inches and different proportions—eighths and sixteenths—in such a way into figures that the calculation could be commenced, would represent an extra number of figures. Thus, instead of the calculation being as was shown in the tabular comparison, there ought to have been many more figures. He understood that the terms under which the paper had been brought before the Institution had reference only to engineering purposes, and he remembered, as a result of the International Exhibition of 1851, that Sir Jos. Whitworth and others, including Mr. (now Sir William) Armstrong, prepared scales to metric measure, and to decimals of feet, carefully and elaborately made. At the same time they were rendered easy of comprehension. One remark in the paper with reference to money opened another question. He supposed it would make very little difference to the British commercially, or as engineers, whether Russia or Turkey ever adopted the metric system or not. He had traveled considerably in countries where the metric system was in operation, and he found that the ordinary people of those countries were enabled to conduct the affairs of every-day life with greater facility, rapidity, and accuracy, than English people could with their own peculiar system. After the first International Exhibition in 1851, so much was said and done about the metric system, that it was almost impossible to elicit a new idea upon the subject. He did not think the arguments that could now be brought forward would enable any one to form a more sound judgment on the matter than could be arrived at from what had been already exhibited by those who had carefully examined the metric system. If it came to be a question of adopting a form of metric system, he thought that a large majority of engineers would carry the day for it. They were dissatisfied

with the present English measure. The difficulties to be encountered, in changing the figures in calculations, represented possible fallacies and inaccuracies, and the greater the number of figures, the greater the number of calculations to be made, thereby increasing the chances of error. He would distinctly express himself in favor of the metric system, or some system which adopted an easy form for calculation, and that the different terms should be called by names that might be readily learned. The thirty-two millions of people in this country, and the Americans, required that names should be given to the different measures, weights and numbers, and values of money, which they could easily understand and remember, rather than terms and names which would be difficult to learn, and, if learned, would soon be forgotten.

Mr. G. Wells Owen observed that it was one of the drawbacks incident to the lot of suffering humanity, a heritage from the days of the Tower of Babel, that people of different nationalities were hampered in their intercourse with each other by differences of language, and although perhaps not coming so directly from that historical event, by differences of measures, moneys and weights. Many of these difficulties and differences had been considerably modified of late years, and perhaps he was not over sanguine in thinking that they would be still more so. The paper, which tended to a discussion upon one mode of modifying some of those difficulties, was one which he thought the Institution might very well discuss, because it was more than thirty years since a discussion on a similar subject took place.

On the 28th of February, 1854, a paper by Mr. James Yates was read before the Institution "On the French System of Measures, Weights and Coins." The discussion upon that paper related more especially to the matter of the decimal coinage, and, although the consensus of opinion seemed to be in favor of a decimal system, the discussion turned to a great extent upon what was the best standard of value as the basis for that decimal coinage. The part that more particularly interested engineers was that concerning weights and measures, and therefore he would allude no further to the coinage, beyond saying that he hoped that if England ever did adopt a decimal coinage she would disregard all questions as to what the present coins were, and effect a radical change by assimilating it to that of the other countries which were members of the Latin Monetary Union. Those who were accustomed to travel where that league prevailed knew how easy it was to pass backward and forward from one country to another without changing their money, and they also knew what difficulties arose the moment they passed from one of those countries into some neighboring State unconnected with the Union. No doubt there would be great inconveniences in adopting an entirely different standard of value, but, although that might be so, it was only necessary to consider what had been done, for instance, in Germany, where, at the time of the formation of the German Empire, the numerous coinages that prevailed in the various States were abolished. Former travelers in Germany would probably remember the difficulties they experienced there. He hoped that if England did adopt a new coinage it would be assimilated to the French franc. The more immediate question before the Institution, however, was that of weights and measures, and perhaps it would be interesting in connection with that if he mentioned the dates at which different countries had adopted the metric system of weights and measures, in order to show how it had spread in recent years. In France, metric weights and measures were enacted in 1795, but were not made compulsory till 1840. The metric system, with Dutch names, was introduced into Holland in 1820, but from the 1st of January, 1870, the metric names came in, with facultative use of the old Dutch names. The next country which took the matter up was a very small one at a considerable distance from France—the kingdom of Greece—where metric weights and measures, but with Greek names, were introduced by an ordinance dated the 26th of October, 1836. In 1852, Portugal decreed that the metric basis should be used, and a period of ten years was fixed for its introduction and adoption. The metric measures of length came into use in January, 1860, weights in July, 1861, surface measures in July,

1862, and measures of capacity in January, 1863. In Spain the metric system was adopted on the 1st of January, 1859, and became compulsory on the 1st of July, 1868. It was also the legal system of all the Spanish Colonies. In Italy the system was adopted in 1859, when most of the Italian States were united to form the Kingdom of Italy. In the German Empire it came into use in 1872, and in the Austro-Hungarian Empire it was made permissive on the 1st of January, 1873, and compulsory on the 1st of January, 1876. In Switzerland it became the legal system on the 1st of January, 1873; it was adopted in Sweden by an Act of the 19th of May, 1875; and in Norway by an Act of the 22d of May, 1875, to come into force at the expiration of three years after the passing of the law. In the United States it became permissive by an act of the 28th of July, 1866. He did not know the date when the system was introduced into Belgium, but it was in use there, and also in the Ionian Islands, Algeria, the United States of Columbia, Peru, Chili, the Argentine Republic, and Uruguay; and it was also permissive in British North America under an Act of 1873. Of course, if the metric system of measures were adopted in England the old systems must be discarded. As the author had stated, the minds of engineers in America and England were saturated, especially with regard to ironwork, with dimensions expressed in English inches and fractions of inches. That would be a great trouble to English engineers at first, but he thought they must not look so much to what would be a trouble to themselves as to what would be a benefit to the world at large, and no doubt in a short time they would get so familiarized with the new measures, as to use them as readily and freely as they now did the old ones. When the German Empire adopted the metric system of measures, sixteen States fused into that Empire had no less than sixteen different lengths for the foot, varying from 9.84 inches to 12.35 inches. Their other measures varied in a similar manner, but the metric system had been adopted throughout Germany without entailing much inconvenience. All specifications in Germany and Austro-Hungary were now prepared on the metric system, and therefore he thought there would not be much difficulty in getting used to it in England, where he hoped it would one day or other, perhaps by degrees, be adopted.

Mr. Percival Fowler said he had been abroad for some years, and had known many engineers well acquainted with the two systems, but he had never met one who, after a month's practice, did not prefer the decimal metric system. He considered that, with the increasing international communication of the present day, it was of the highest importance that weights, measures, &c., should be uniform in all countries. Scientists had long since recognized the convenience of an international technical language, and in biology, botany, and many other sciences, Latin or Greek names had been chosen which were intelligible to all the civilized world; in electricity the new nomenclature had been adopted by an International Commission. Considering that a uniform decimal system had been adopted in nearly all civilized countries, it was necessary that England should seriously consider the question. English machinery went all over the world, and had to be mounted in other countries by workmen who were not acquainted with eighths, sixteenths, full thirty-seconds, and other like measures. In England the decimal system had been used in many ways. Barometers were graduated in inches and tenths of an inch, level-staffs to feet and tenths of a foot; and in spite of ourselves the decimal system was being introduced into this country. The difficulty of learning the metric system had been immensely overrated. He could speak from practical experience that in Spain, in a week, the workmen became thoroughly conversant with meters, centimeters and millimeters, and he was certain that the difficulty of changing the English system of weights and measures to the decimal system had been greatly overestimated. He had heard it said that English weights and measures were more convenient, but he did not see how that could be. He believed that one Ordnance scale was $\frac{1}{2500}$, and besides the poles, yards, feet and inches, the land measure was 7.92 inches in a link, 100 links in a chain, and 80 chains to a mile. Silver and lead ores were reckoned by so many ounces troy to 1 ton avoirdupois,

so that in order to arrive at the proportion of silver in lead it was necessary to take two separate measures, and calculate them. With regard to the question of calculations for water, since a liter of water weighed a kilogram, the calculation was of the simplest sort. In England the measurement was effected in cubic feet or gallons, and he always had to refer to a book of tables, for he could never remember the equivalent figures. A most inconvenient scale was three-sixteenths of an inch to a foot, and a calculation had to be made as to what proportion that was. He did not understand why it was that Englishmen had not yet adopted the decimal system. As for British pride, the French proverb might be true "Les Français inventent, mais les Anglais perfectionnent," but as regarded the decimal system, Englishmen had neither invented nor perfected it, and he thought they ought to adopt what had been found practicable in nearly all civilized countries. It would be of the greatest convenience, as he did not believe that a single member of the Institution had not constantly to refer to a table to convert English weights and measures to their metric equivalents.

Mr. Rodolph De Salis desired to mention a few facts which he thought told in favor of the present systems. It could not be denied that starting with a clean slate a universal system of weights, measures and coinage would be desirable; but it might be fairly questioned whether a decimal system would be the best, and if a decimal system were adopted, whether it should be based on the meter; and as different systems were in existence, it might also be questioned whether the advantages of a change would counterbalance the disadvantages. In considering such a change, the advantages should be gauged not by the preferences of scientific persons, but by the convenience of the bulk of the people; and he certainly thought, for general purposes, a system admitting of binary and ternary subdivisions was more convenient than a system based on decimal subdivisions. All knew the convenience of dividing anything into halves, quarters, or thirds, or their subdivisions. It was not so clear how conveniently to divide things into fifths, tenths, and hundredths in the ordinary practical affairs of life. Of course, for all purposes of calculation a decimal system had immense advantages; but, practically, that system was in use in all calculations. Everything was decimalized, or was worked by logarithms. Even actuarial computations were always made in sovereigns and decimals of sovereigns; indeed, under the present system, computors had the advantage of using decimals when they liked, and fractions when they were most convenient. The present measures, he thought, were very convenient. A yard, for example, was the $\frac{1}{1760}$th of a mile, and the $\frac{1}{4840}$th of an acre. Then there was the ternary division of the yard, the foot. He had always considered the foot to be a particularly handy measure for hydraulic and other calculations. It correlated very well with the gallon and the pound, 25 quarter gallons or 50 pints making a cubic foot—near enough for all practical purposes—and 10 lbs. of water a gallon.

With regard to the practical uses of the foot, the only instance he would give was that of building, and the cognate trade of brickmaking. A brick was $\frac{3}{4}$ by $\frac{1}{4}$ of a foot, and its breadth was its length divided binarily. Those were convenient fractions, and from them quantities could be estimated. The thickness of a wall was one brick, a brick and a-half or two bricks, and it contained so many courses of brick-work, all in terms of the foot. If the metric system were adopted instead, it would be found that a brick was equal to 0.2286 by 0.0762 by 0.1143 of a meter, which he did not think would be very convenient. But if the brick were to be changed to suit the new system, the inconvenience would be very great; all contracts for the sale of brick earth would have to be revised, and brick-making machinery would become antiquated. Again, it would be a great disadvantage that new work could not be conveniently joined to old work. The old bricks could not be used with the bricks of the new standard. How could old work be bonded on to new? It would be almost sure to result in failure. With regard to the theoretical basis, he agreed with the author that the accuracy or otherwise of its relation to the meridian was not material; but according to Sir John Herschel, the inch was as scientific a basis as the meter. Again, the yard was an extremely convenient step,

and on that account a good basis. It was a better step than the meter. It was a natural step for a tall man, and a very long step for a short man; but adding 3.37 inches for each step would probably put it beyond the compass of a short man, and he could state personally that it was a longish step for a tall man. That was a practical consideration which ought not to be disregarded. What were the advantages to be gained by the adoption of a change which would certainly affect nearly every transaction in life? A map had been exhibited showing the size of the countries using metric measures as compared with the British Islands, and on the theory that the smaller must yield to the greater, it was urged that the metric system should be adopted in England. That argument, if worth anything, operated entirely the other way. The British sway extended over one-fifth of the habitable globe, while the area of metric countries was only about one-thirty-fifth. The British Empire included a population of 310 millions, the United States 50 millions, and the metric countries about 200 millions. The British imports and exports for 1883 to the British possessions amounted to £188,000,000, to the United States to £135,000,000, to various other places, to £142,000,000, making a total of £465,000,000; the amount to the metric countries was £266,000,000. Therefore, as far as size went, the balance was entirely on the side of the present measures. The coinage would certainly be more conveniently affected by a change than weights and measures; but he thought the commercial classes were not in favor of change. In 1849 the florin, the decimal subdivision of a pound, was introduced, and the coinage of half-crowns was stopped. In 1874, a circular was addressed to bankers and others interested, asking whether half-crowns should be discontinued or florins, or whether both should be coined. In official phraseology the answer to the circular showed an "overwhelming preponderance" in favor of the continuance of both coins, while out of six letters to the *Times* on the subject, not one advocated the dropping of the half-crown. He did not regard with the author's equanimity the depreciation in the value of their library which would result from a change in notation. No doubt a certain number of text-books would be translated, but the bulk of the works, all the Parliamentary papers, books of reference, and, he should imagine, nineteen-twentieths of the library would become antiquated and practically unusable; whereas, under the present system, it was found that any good book published in French or German was promptly translated into English with English measures, and took its place beside English literature. It had been urged that the metric or decimal system was much easier to learn. No doubt it was, but surely that was not altogether an advantage. He would put in the form of a proportion sum: As Greek is to French so is the English complicated system to the metric system in point of mental training. He dissented altogether from the idea that the professional classes should be the first to advocate a change of that sort. Such a change ought to come from the bottom and not from the top, and ought to be the product of necessity. That the bulk of the people did not feel the need of the change was, he thought, shown by the fact stated by the author, that the general public in both countries, England and the United States of America, appeared indifferent on the subject. As professed mathematicians, or at least arithmeticians, he thought engineers ought to be ready to adapt themselves to any system that appeared to commend itself to their clients, and not expect them to give up convenient binary divisions in order to facilitate professional calculations. He classed a universal system of measures, weights, and money, in the same category with a universal language. Both were theoretically very advisable, but he thought they were not practicable. No doubt they would be much less anomalous than the present system, but he doubted whether they would be so useful.

Mr. R. C. Rapier said that, ten years ago he should have made much the same speech as that which had just been delivered. In 1868, when the great discussion on the subject took place, he was strongly in favor of retaining things as they were; he then thought there was no measure of length so convenient as the English foot, and no unit of capacity so useful as the English gallon. But circumstances altered cases. During the last fifteen years he had been obliged to have recourse to

meters, kilometers, and such like measures, and he found that they were not so objectionable after all; in fact, he had compiled some useful and handy rules for himself on the subject. Mr. De Salis had spoken of the importance of conserving the English system. Mr. Rapier ventured to say that the English weights and measures were not a system, they were an accident; they were never created upon any systematic principle or plan. In one part of the country 14 lbs. were a stone, and in another 16 lbs., and similar diversities were well known. Whether the standard of length was a pendulum vibrating seconds, or the common yard or foot, he did not care; but he maintained that a decimal system was the right one, and he inclined strongly to the metric system, because it was already established over the rest of Europe. The metric system had the advantage of an intelligible and definite connection between units of length, units of capacity, and units of weight. The meter was referable to the vibration of a pendulum; so would any other unit be, but it so happened that the cubic meter coincided very nearly with the English ton, the French tonne. He thought encouragement might be taken from the fact of that connection. With regard to money, the present system on the plan shadowed forth by the author, and previously mentioned by Sir John Herschel, admitted readily of an adaptation to a decimal sytem, and it had a tolerably near affinity to the system on the Continent, bearing in mind that twenty English shillings were as good as twenty-five francs. The convenience of the relationship of the measures on the metric system was very great. For instance, a cubic meter of water was a ton. The number of cubic meters of any other material multiplied by its specific gravity gave the number of tons. Again, to take a familiar illustration, that of a railway to be laid with rails 35 kilograms per meter, there were 70 metric tons in a kilometer; it was easy to reckon how many English tons there would be according to the English system, but in the other case it was obvious. If the members would regard the matter practically, for about six months, they would begin to think in the new measures, and then the whole would become perfectly easy. To facilitate thinking in metric measures, he had adopted the following ready rules: To convert meters into feet, divide by 3, and move the decimal point one place to the right. To convert feet into meters, multiply by 3, and move the decimal point one place to the left. To convert millimeters into inches, multiply by 4, and move the decimal point one place to the right. To convert inches into millimeters, divide by 4 and move the decimal point one place to the left. To change hectares into acres, divide by 4, and move the decimal point one place to the right. To convert square meters into square yards, add one-fifth. To change cubic meters into cubic yards, add one-third. To convert kilometers into miles, divide by 2, and add one-fourth. All the above examples were worked by the use of the elementary numbers, 3, 4, and 5, and gave results correct within $1\frac{1}{2}$ per cent., and the rules were therefore sufficiently near for the purpose of mental calculation, to facilitate a familiarity with the metric equivalents.

Mr. E. W. Young, to show the comparison between decimals and duodecimals, exhibited a calculation in compound addition by both methods, in which there was one column more in the decimal sum than in the duodecimal. Again, to illustrate the ordinary transactions in commerce, millions of which took place every day in London alone, suppose a shopman wished to ascertain the cost of 17 yards at $5\frac{3}{4}d.$, he did not reduce $5\frac{3}{4}d.$ to farthings, multiply by 17, and reduce back to pence and shillings, but he applied a process of mental arithmetic, thus: he said, Why, that is 17 sixpences less 17 farthings, or 8*s.* 6*d.* less $4\frac{1}{4}d.$, that is 8*s.* $1\frac{3}{4}d.$ It was done in an instant. Now if the decimal system were in force, and there were 25 farthings to the sixpence, the shopman would have to multiply yards by farthings, which would be far less convenient than the present system. Indeed, for every little purchase made, the shopman would have to pull out pencil and paper to make a calculation. The duodecimal system had the advantage over the decimal system in admitting the use of fractions to a much greater extent, so that sums could be more easily done mentally. It was not the convenience of the scientific or professional man in his office that ought to be considered, but the general convenience of the multitude in the millions of transactions going on throughout the

country. The introduction of the meter would necessarily mean the introduction of the decimal system generally, and he thought that an alteration to the decimal system would occasion very great inconvenience, and it was exceedingly doubtful whether there would be any economy of labor in it. For the purpose of contrasting the meter with the present British standards of measurement, the following example afforded a fair comparison between the two systems. The sum was: required the contents in cubic yards of a block of masonry 88 feet by 3 feet 9 inches by 2 feet 3 inches. The usual way of doing this was by fractions, thus, $\frac{88^{y}}{3} \times 1\frac{1}{4}^{y} \times \frac{3}{4}^{y}$. The threes canceled one another and the result was at once $\frac{11}{4}^{0}$ cubic yards $=27.5$ cubic yards. Using the decimal system, it would be necessary first to multiply 88 by 3.75, and the product by 2.25, involving a large number of figures. Even then the result would only be in cubic feet, and though it would be unfair to the decimal system to debit it with the figures that would be needed to reduce cubic feet to cubic yards, the sum so done would be a better comparison of the two systems than the example given by the author, which he thought ingeniously unfair. With regard to the suggestion that twenty-five farthings would equal sixpence; that, he thought, would be a great disaster, because 24 could be divided by 2, 4, 8, 12, 6, and 3, and those numbers were again divisible by others. Altogether there were eighteen divisions and subdivisions, but 25 could only be divided once by 5. He did not know how it was that the present duodecimal system was first hit upon. Evidently those who made 12 inches to the foot had some idea of utility. The same system was constantly used. There were twenty-four hours to the day; the arc was divided into 360°, a multiple of twelve. He had heard, however, that an agitation was going on for a division of the circle into decimals, which he should very much regret. If that were accomplished he supposed there would be a demand for twenty hours to the day instead of twenty-four. There was a disadvantage in the decimal system in consequence of the similarity of some of the terms, such as decimeter, and decameter, deciliter, and decaliter, deciare and decare, decistere and decastere. He believed that working men would be confused with words so much alike, and a great many mistakes would be made through bad writing. If a man forgot to dot the "i" that might make all the difference between a decimeter and a decameter. He thought it would be a good thing to get rid of the 66-foot chain. There were many anomalies in the present system, and a great many things that it might be desirable to change; but it was a serious matter to propose to change the whole of the system adopted in England, in the Colonies, and in the United States. He considered it a mistake to lay so much stress upon foreign commerce. No doubt it was large, but it was small in comparison with the internal commerce of the country. Foreign commerce, too, was wholesale, while internal commerce was chiefly retail; so that there might be ten thousand or one hundred thousand transactions taking place in this country with the present system of coinage and measures for one negotiation in meters. When Englishmen went abroad they took French money with them, and knew how to deal with it, and on returning home they had no difficulty in reverting to their own system. It might be necessary for German duchies about the size of an English county, placed between two large States like France and Germany, to adopt the metric system, but not in a country like England.

Mr. C. E. Cowper wished to call attention to three points. With reference to the calculation submitted by the author, some of the advocates of the British system had computed it in different ways, some by duodecimals and some by vulgar fractions. The author had brought it forward as a sample calculation to show the superior advantage of the metric system, but he did not think he had done his best; because, though the tank might have been marked on the drawing 10 feet 6 inches, its dimensions would be, perhaps, when made, $\frac{3}{8}$ inch more or less than that length. What was a tank to be measured for? If he had to measure the tank to find the weight of the water, in order to ascertain what sized injector to put in, or what size of feed-pump to use for a boiler, he would be content to take it approximately—say 2 tons, which would be near enough. Suppose, on the other hand, great accuracy

were needed—if, for example, the tank were used for gauging water in a pumping-engine trial, measuring the feed-water into the boiler (and the contract price might depend on the economy), the case would be very different. In that case the dimensions of the tank might be 10 feet $6\frac{3}{8}$ inches by 6 feet $2\frac{5}{8}$ inches, and the depth of the water, perhaps, 1 foot $\frac{3}{16}$ inch. He thought the duodecimal system, or the vulgar fraction system, would not then work out so neatly; but by the metric system it did not matter whether the measurement was 0.33, 0.34 or 0.35; the calculation was not any longer. Again, the author might, he thought, have made more of the smallness of the unit. Regarding the question as a matter of abstract principle, suppose it were necessary to make an arrangement for a new world, where no difficulties would arise on account of established customs or international intercourse, a small unit of measurement, convenient to work to, and one that could be easily divided on boxwood or steel, would certainly be looked for. Such a unit existed in the millimeter, but not in the inch, or any decimal division of the inch or foot; the tenth of an inch was too large, and the one-hundredth too small. The millimeter was as small as required for any practical purposes, and it could be subdivided microscopically to any extent. Reference had been made to the use of logarithms, but he wished now to direct attention to the mechanical use of logarithms, to which, he contended, the metric system lent itself. He might raise a smile if he referred to the slide rule, because people generally thought of the dirty, black, greasy thing which the fitter carried in his pocket and did not know how to use; the slide rule there was in the wrong place; the fitter did not want it, and the proper place for it was in the office. It was little used in England, and one of the reasons for that was the complicated English system, or combination of systems, of weights and measures. In France, one of the improved forms of the slide rule was found, he had been informed, on almost every engineer's desk; in electrical engineering it was very largely employed. Two of the largest electrical establishments in England used one or other form of the slide rule, or the logarithm slide, to a great extent. He therefore claimed that the metric system, being a decimal system, which allowed every calculation to be checked readily by the slide rule, possessed a substantial advantage. Many persons thought this instrument a trivial affair, but only a practical acquaintance with it would enable them to form a sound judgment as to its merits.

Mr. H. J. Chaney thought it was desirable, in order to facilitate discussion, to separate the question of weights and measures from that of money. The money aspect was no doubt very interesting, but experience had shown that the question of the introduction of a new system of weights and measures could be best considered by itself. The subject under discussion was not that of decimalizing weights and measures, or introducing a decimal system of money, but the desirability of introducing the metric system, which was not only decimal, but binary. He supposed that few persons who had much acquaintance with the present system of weights and measures would admit it to be the best that could be provided. The metric system, *per se*, was by far the most convenient; but then the inconvenience of making a change which would so largely affect the daily transactions of life had to be considered. The discussion, he thought should deal with the difficulties in the way of such a change. The question of the comparative advantages of the metric and British Imperial systems had been well thrashed out. Reference had been made to the Standing Orders requiring that all scales should be according to Imperial measure, as 4 inches to the mile, and he would suggest for consideration the nearest form of metric equivalent. Besides 4 inches to the mile, the Standing Orders might also permit a scale of 65 millimeters to the kilometer. Buildings, also, were to be drawn to an enlarged scale, of not less than $\frac{1}{4}$ inch to every 100 feet; for that scale there might also be permitted a scale of 7 millimeters to every 30 meters. Sections might be given to a scale of 1 millimeter in every 1,200 millimeters, or in every 12 decimeters, as well as to a scale of 1 inch to every 100 feet. Quantities might be allowed to be given in meters and parts of a meter, as well as

in yards, feet, decimal parts of a foot, and inches. Distances might be permitted to be expressed in kilometers and meters, as well as in miles, furlongs, chains, poles, yards and feet.

Mr. W. Walton Williams said he did not think it was necessary to choose immediately between the two systems. Engineers before tying themselves down to the metric system, ought to consider whether it could be in any way improved. According to French engineers the metric system was absolute perfection, and Englishmen were a set of idiots for not adopting it. He had been obliged to use the metric system for many years abroad, and he desired to point out two or three disadvantages connected with it, in the hope that they might be remedied. No man could step a long distance in meters. He had seen French engineers try to do so; they could step 100 meters, but if the distance was 400 or 500 meters, they would reckon every step to be 90 centimeters, equivalent to the English yard. The author had stated that the meter was the right length for a measure, but Mr. Williams contended that no man could measure with it easily. The eye subtended an angle of 60°, so that a man's arms ought to be a meter long to be able to see both ends of the meter rule. The author had stated that a workman could measure with a meter rule in two-thirds of the time required with a two-foot rule. That he denied from his experience of foreign workmen, who, with a meter measure, were generally longer and less accurate measuring than Englishmen with a two-foot rule; besides which the foreigner generally called in another workman to put a mark at the end. The French themselves did not admit that the meter was absolutely the best unit of length, for any one going into a draper's shop would find that a half-meter and not a meter measure was generally used; the demi-kilo was frequently used for weight, and the customer might be heard asking for "Le quart d'un demi-kilo." With reference to leveling, it did not make much difference theoretically whether engineers divided a foot or a meter by the decimal system, except that they could frequently see the foot mark on a staff, when they could not see the meter mark. He had known instances in which a mistake had been made of a whole meter, because a man would not, or did not, lift up the staff the required distance. If a French staff with a disk were used it would be different, but where the staffman moved the disk up and down, it had to be brought each time to be verified, so that the time was doubled. With regard to drawings, if they were made to a scale of $\frac{1}{10000}$, $\frac{1}{5000}$, or even $\frac{1}{1000}$, it did not matter whether feet or meters were used. In drawing to a larger scale, 1 inch to the foot, the English $\frac{1}{12}$ corresponded to the French $\frac{1}{10}$; but to vary it the Englishman could go from 1 inch to 1½ inch, or to ¾ of an inch, while the Frenchman would have to go up to ½ or down to $\frac{1}{20}$. It had been said that $\frac{3}{16}$ of an inch was an absurd scale. Possibly, but it was $\frac{1}{64}$ of a foot, and 64 was 2 multiplied by itself five times. To put $\frac{1}{64}$ of a meter into decimals, six places of decimals would be required. He did not say that engineers ought not to adopt the metric system, but they should not be absolutely tied down to it as it at present existed. It was their duty to improve it, if possible, according to the saying already quoted: Les Francais inventent, mais les Anglais perfectionnent." He wished to address a few words to those who were in the habit of preparing drawings for foreign works. He had had many such drawings sent out from England, and they had been usually drawn to an English scale, and the dimensions inserted in meters. When a Spanish stonemason or an Italian carpenter saw such drawing he was puzzled, and fresh ones had to be prepared for his use. To those who were in the habit of using the 1-inch or 1½-inch scale, $\frac{1}{10}$ was not convenient, but he thought that, considering the immense number of drawings going abroad for foreign work, a little sacrifice ought to be made in order to adapt them to the metric system, even if that system were not generally adopted in this country.

Mr. W. H. Thelwall remarked that he had used both the metric and the English systems for nearly twenty years, and although he was not frightened at the metric system, he was decidedly of opinion that the English was much more convenient in all matters of civil engineering. The author had not given any statement of the relative times taken to do various works. He had said that a

French workman could measure planks in two-thirds of the time occupied by an English workman, but if that statement was to be of any value it ought to be supported by actual experiment. He fully agreed with what had been urged by Mr. Walton Williams that the meter was inconveniently long for accurate measurement. The author's statement with regard to leveling was much too vague to be of any practical value, since it contained no account of the time saved. According to his own experience, French engineers were not satisfied with working to centimeters, but even in trial sections over rough ground worked to millimeters. In the case of sections of railways, the ground and formation levels and the heights and depths of cuttings and banks had been all worked out to millimeters, requiring three places of decimals instead of two, which was a serious matter. Referring all measures to the meter involved the use of a much larger number of figures than the English system. He had copied some dimensions, that he had recently met with in a French publication, of a tramway locomotive. The diameter of the cylinder was 0.220 m., in English measure it would be 9 inches; the stroke of the piston was 0.350 m., in English it would be 14 inches; the weight, empty, was 11,000 kilograms, in English, 11 tons; the weight, running, 13,000 kilograms, in English 13 tons; the heating surface, 18.90 square meters, in English, 203 square feet. Thus, there were twenty-two figures in French, and only ten in English, measure. Take another instance, that of the strains and weight of a wrought-iron pillar, having a load of 25 square meters of flooring weighing 6,600 kilograms per square meter. $25 \times 6{,}600 = 165{,}000$ kilograms. The area of the pillar was—

Web.....	450×12	= 5,400	sq. millimeters.
4 plates.....	500×12	= 24,000	" "
4 angles.....	$\frac{100 \times 100}{12}$	= 9,000	" "
4 "	$\frac{80 \times 80}{10}$	= 6,000	" "
Total....		44,000	

Load per sq. millimeter $\frac{165{,}000}{44{,}000} = 3.7$ kilograms.

In English measures the above would be, load of 270 square feet of flooring weighing 0.6 ton per square foot $= 270 \times 0.6 = 162$ tons. The area of the pillar would be—

	Inches.		Inches.		Inch.		Sq. Inches.
Web.....	18	×	$\frac{1}{2}$			=	9
4 plates.....	19	×	$\frac{1}{2}$			=	38
4 angles.....	4	×	4	×	$\frac{1}{2}$	=	15
4 "	3	×	3	×	$\frac{3}{8}$	=	8
Total....							70

Load per square inch $= \frac{162}{70}$ 2.32 tons.

Such examples might be multiplied indefinitely. They had not been made up for the purpose of showing comparisons unfavorable to the metric system, but were fair samples taken at random from actual French practice, and converted into the corresponding English figures. Again, such things as diameters of pipes were measured by 200 millimeters, 150 millimeters, and so on, the English equivalent for which would be 8 inches and 6 inches. It frequently happened that, in the French system, three figures were used instead of one; and when in taking out quantities the figures had to be multiplied, the difference was very serious. If the author really considered that there was a saving of time by the metric system, he ought to have given an example, such as the estimate of work, or the calculations and quantities in a large iron bridge properly worked out by both the metric and the English systems, and not the long sum in the Appendix, which could be computed with less than half the number of figures given. He was convinced that the more acquaintance English engineers had with the practical working of the metric system, the less disposed they would be to exchange their own for it.

Mr. W. Airy thought that the author had somewhat mixed up two very different matters, the metrical unit and the decimal system, which ought to be kept distinct. It appeared to him that the best unit was that which the experience of people had decided to be the best for the greatest number of measurements. For the great bulk of measurements—those relating to building, construction, and manufacturers generally—the meter could not be used without subdivision, and it was therefore, in his opinion, wholly unfit for the unit for such measurements. That was proved by the fact that, in England, where the people had

the choice both of the yard and the foot —and the yard for that purpose might be considered as comparable with the meter—for the great bulk of their measurements they used the foot, and not the yard. He thought it would hardly be denied that the foot was the real unit of the English people. The meter was much too long a unit for the ordinary affairs of life; and even in France, the headquarters of the meter, there were cases in which the French still used the foot. In Sweden and Norway also, as well as in other countries professing to use the metric system, the foot was employed for a variety of purposes. He thought there could be no doubt that the decimal subdivisions of the unit, whatever it might be, afforded great facilities for calculation; and therefore for all those measures, or classes of measures, which involved frequent or complex calculations, great advantages would be derived from the adoption of that method. It would, for example, be a great advantage to subdivide the foot into 10 inches, and the inches into tenths, and he thought that such a change would be acceptable to the country at large; but for all measures referring to articles of consumption, he believed the binary system, which was much more simple to uneducated people than the decimal system, had a firm hold upon the people, which it would be impossible to shake. The author would have done well to have made some mention of the case of Russia, which, long after the metric system was fairly established in central Europe, had occasion to set her standards in order, and after due consideration had adopted the English foot as the unit, altering the national measures so as to form exact multiples of it.

Mr. E. B. Hanson said he thought that the sum, given in the duodecimal system, ought to have been worked by duodecimals and not by decimals. In answer to Mr. Airy, it would be sufficient to say that the standard yard was divided into feet, of which 1 foot was divided into 12 inches, and so the foot formed part of the standard. The author had found fault with engineers for measuring with a two-foot rule, and afterwards asked them to measure with a 20 meter chain, which was hardly consistent. He then spoke of "five-sixteenths full," or "bare three-eighths," as if they were the same thing. Mr. Hanson had always understood that if there was a hole of exactly three-eighths diameter, the cylinder which would fit into it would be a "bare three-eighths," and *vice versa*. He did not think that any brickwork or any engineering work was measured in rods now; he believed it was always measured in cubic yards. A scale $\frac{3}{16}$ inch to a foot was, he believed, one of the best. It came near to five feet to the inch, and it had the advantage that it was a fractional part of $1\frac{1}{2}$ inch to the foot, which was so useful a scale to the carpenter, inasmuch as $\frac{1}{2}$ inch on his rule represented an inch. Ordinary working plans for railways and so on were plotted with the horizontal scale 2 chains, and the vertical scale 20 feet to an inch respectively. There was not the slightest difficulty in passing from one to the other. The same scale did for both, whereas with the metric system two scales would be necessary to measure by, and there would be a constant changing. If parliamentary deposited plans were plotted in meters, farmers and others would be wanting to know how many square meters there were in an acre. They did not reckon by meters, and it would take years to make them do so. When he was in Paris during the Exhibition of 1878, he went with a brother engineer to visit some shops where several young artisans were being taught under the French Government, and he was much astonished to find that they were using the two-foot rule, and also using English names for the various tools. On asking the reason, he was told that it was found to be so much more convenient. In some shops in England where a large number of Germans were employed, machines had been constructed partly in millimeters and partly in inches. Pulleys and things of that sort were generally measured in inches, and the smaller parts in millimeters. Considering that the meter was introduced into France forty-five years before it was properly legalized, before railways were made, and before engineering works assumed their present importance, the time it would take to introduce it into England now could hardly be estimated. There were certain standards in England which might be considered as hard and fast. The railway gauge was

one of them, and it could never be altered. The measurement of 4 feet 8½ inches looked well enough as it was, but in the new system it would be 1.435 m. There was also a standard measurement of bricks. If the metric system were introduced, he supposed that bricks would be made ¼ meter long. A trial had been made of bricks 10 inches by 5 inches by 3 inches, but they had not succeeded, and he believed the use of them had ceased.

Mr. L. F. Vernon Harcourt said there could be no doubt that scientifically the metric system, in which the weights and measures were connected, and arranged according to a definite plan, was better than the English; but there were some disadvantages in it which ought not to be overlooked. The meter, being a larger unit than a foot, required more subdivision to measure small dimensions, and was less well adapted for leveling than feet and decimals of a foot. The constant use of decimals, moreover, in the metric system, led to the abandonment of fractions and proportions, which had a distinct value of their own in certain cases. Errors, for instance, were more liable to occur when gradients were expressed in decimals, such as 0.00125 and 0.000333, instead of as 1 in 800 and 1 in 3,000; and $\frac{3}{10}$ and $\frac{1}{6000}$, were simpler modes of expressing these proportions than 0.1875 and 0.000016. In a string of noughts after the decimal point, one of them was very liable to be omitted, or the decimal point itself might be readily misplaced, causing in either case a tenfold error. Also fractions were expressed more shortly in words, and were consequently more easily remembered and repeated than decimals, where each digit had to be separately named; and the use of commas for marking off the thousands, which greatly facilitated the reading of figures, could not be employed for decimals. The author had stated that scales for drawings were much better represented by the French than by the English system, and Mr. Vernon-Harcourt had therefore taken the trouble of looking out a few scales given in some French engineering works, of which the following were instances of those occasionally adopted; namely, 0.06667 m. per kilometer, 0.00143 m. per meter, 0.004165 m. per 100 meters, 0.0104 m. per meter, and 0.0278 m. per kilometer. Those figures, he thought, would be sufficient to show that the French scales were not always so simple as some persons imagined. No doubt a reform might be introduced, and it would be a great advantage in all engineering literature to have scales that would be comparable. He had himself tried to introduce such scales in the form of definite simple fractions of the natural size. In the illustrations for a book very shortly to be published, he had managed to make two hundred and twenty-five figures to nine scales, all of these scales being multiples, or parts of one another, ranging between one thirty-thousandth and one hundredth of the full size; so that it was possible to compare the various plans together. As to the statement with reference to calculation, he felt sure that any one who had worked with Bidder's tables for getting out earthwork could do so more quickly that way than by any other method. The author had stated that in the case of English measurements, bushels, and cubic yards were used. When inspecting works in France he had frequently asked for the proportions of cement or lime employed in concrete or mortar, and he was always told that there were so many kilos to the cubic meter; so that in that respect there was not much advantage in the French system over the English. If the new system were adopted in parliamentary plans, especially if some plans were figured in meters and others in the ordinary English dimensions, both witnesses and the legal profession would be much puzzled. It was questionable whether the advantage in consulting foreign books, that would accrue from the adoption of the metric system, would for a long time counterbalance the loss resulting from the different notation of the whole existing British and American engineering literature. Much of this literature, including the eighty volumes of Minutes of Proceedings of the Institution, could not be reproduced with metric measures; and the necessary alterations in the Ordnance survey and the Admiralty charts would involve a large expenditure. If the metric system were introduced it would simplify calculations; but, in making the change, it would be necessary to take into account

all the English-speaking communities, and a conference would be required in order to arrive at a consensus of opinion. A common system of weights and measures would be of very great value. The discussion, however, had indicated that much could be urged on both sides of the question; and unless the balance of advantage proved greatly in favor of the change, the present generation could hardly be forced to incur great inconvenience for the possible benefit of posterity. The change, if made, should be gradual, with the concurrent use for some time of both systems, and aided by elaborate conversion tables. Moreover, in such a matter, Great Britain could not be regarded merely as a European power, and the metric system would have to be adopted also simultaneously by the Colonies and the United States if any real benefit was to be gained.

Mr. J. W. Barry agreed with Mr. Airy that the meter was far too large a unit for practical work. Some portion of his early life having been passed in measuring up work with quantity surveyors, he could bear out the statement that in no case was the yard used as the unit of measurement, and in the case even of large dimensions, the foot was universally employed. He could by no means adopt the recommendation of Mr. Airy, that the foot should be divided into ten parts. It was a very great advantage that it was divided into twelve parts. Any one who had had much practical work would know the advantage of being able to divide the foot by two, three, four and six. The decimal system was comparatively useless to a man who had to work with his hands, or for mental calculations, and, moreover, absolute correctness could not in many cases be obtained by it. By the duodecimal system absolute correctness could always be obtained. Very often a measurement was required which had, perhaps, to be afterwards multiplied many hundreds of times, and if there was a small fraction wrong in the original measurement it might result in a serious error. The change from the duodecimal to a decimal system would, in his opinion, be a retrograde movement. He thought nothing was to be gained by discarding duodecimals, and would say, in the words of Lord Melbourne, "Why cannot you let it alone?"

COMBUSTION UNDER PRESSURE.

From "The Engineer."

THE conditions determining the economical efficiency of steam boilers are so complex that they defy the theorist. It is quite possible to lay down certain premises and deduce from them that the efficiency of a steam generator cannot be greater than a given maximum, and this deduction will be true; but it is quite impossible to say what is the minimum below which the efficiency of the boiler, expressed in terms of pounds of water evaporated by a pound of coal, cannot fall. Between the worst and the best possible performance there is a wide tract, and it is in this tract that the engineer finds at once a field for his labors, and puzzles which he can only solve with difficulty. At the present moment the improvement of the marine boiler is a prominent topic, of conversation, at least, with marine engineers and shipowners. Hitherto it seems that in only one direction is economy being sought, namely, in the adoption of forced draught. We venture to think, however, that there are more ways than this in which the economic efficiency of boilers may be improved.

There can be no doubt that the relative dimensions of grate surface, tube surface, and calorimeter area—that is to say, the cross-section of the tubes—play a very important part in determining the efficiency of the boiler. Numbers of instances of this may be cited. Not less important is a point to which little attention is paid, namely, the thickness of the fires. That is a matter left to the stokers, and settled without any reference to the peculiarities of the coal. Yet turning to the Wigan experiments made in 1867, we

said that the reporters, Mr. Thomas Richardson, M. A., and Mr. Lavington Fletcher, say: "The thickness of the fuel on the grates has proved to be an important element in the proper management of north-country coals. We have tried 9 in., 12 in., and 14 in. fires, and in all instances, whatever were the other conditions, the greater the thickness of the fires the more speed and power were obtained from the coals." To illustrate this we may add that "Great 7 ft." coal with 9 in. fires evaporated 9.779 lbs. of water per pound of coal, at the rate of 48 cubic feet per hour, while the same coal with 14 in. fires evaporated 10.494 lbs. at the rate of 53.3 cubic feet per hour. "Black rod yard," with 9 in. fires, evaporated 10.236 lbs. at the rate of 44.74 cubic feet, and with 14 in. fires, 11.057 lbs. at the rate of 45.36 cubic feet per hour. It is sometimes assumed that, with a small grate a thick fire must be carried, or the required quantity of coal cannot be burned in a given time; but this is only partially true. However, it is usually the case in practice that the smaller the grate the heavier is the fire.

Concerning calorimeter, there is reason to believe that it is often too large. We wish first, however, to note an experiment made years ago in the United States, by Isherwood, to determine the effect of various calorimeters on the economic efficiency of a marine boiler. This investigation showed that when the area of the tube opening—calorimeter—was in the ratio of 1 to 11 of grate area, the boiler evaporated 8.57 lbs. of water per pound of anthracite, but by reducing the calorimeter to $\frac{1}{15}$ of the grate area, the economic efficiency was increased by nearly 15 per cent., but the power of the boiler was greatly diminished, because even with the aid of a steam jet, not more than a little over 5 lbs. of coal could be consumed per foot of grate per hour. It is to be regretted that the experiment was not pushed further with the aid of a fan. It appears, indeed, to be certain that the calorimeter which will best suit a natural draught is not that best adapted to a forced draught. For example, if the velocity with which the products of combustion are passed through the tubes, be doubled, the quantity of air driven through the fire being doubled, no economic advantage can be gained, because the hot gas will not have time to part with its heat; and the proof that this is the case is supplied by the tremendous smoke-box temperatures, from 1100 deg. to 1300 deg., obtained when the forced blast is used. There is reason to think that a direct and considerable advantage can be gained by burning fuel under pressure. Why, is by no means clear. Apparenty, the proper method of working a boiler with forced blast is to obstruct the smoke-box end of the tubes with thick ferrules. The gases would rush through these at a very high velocity, while in the rest of the tube their motion would be comparatively slow, and some amount of whirling would be set up which would be highly advantageous. One of the great defects of the marine and locomotive boiler is that the products of combustion move in lines parallel with the tubes, whereas, under all circumstances, the greatest value is got out of heating surfaces when the hot gases strike them at right angles. In no case, however, can too much care be taken to break up the products of combustion and mix them, so that, as Peclet has shown, hot and cold layers may not be formed. The marine boiler is fairly well designed in this respect, because the combustion chamber serves as a mixing chamber before the gas enters the flues. There is reason to think, however, that a type of boiler much used in the United States, and in a modified form, by Mr. Holt, of Liverpool, is better adapted for forced combustion than is the ordinary boiler. In the boiler we refer to, the products of combustion first pass away to the combustion chamber through a number of tubes about 12 in. in diameter, the ordinary 3½ in. or 4 in. tubes returning above them and the furnace in the ordinary way. It is held, we know, that, space for space, this boiler is not so powerful as the ordinary type, but it must not be forgotten that one result of working with forced draught will be, other things being equal, to augment the absolute as well as the economic efficiency of steam generators; so that the objection just stated seems to fall to the ground.

It is much to be desired that some competent firm shall carry out a series of experiments on the influence of the calorimeter, or, more exactly, on the value of combustion under pressure. There would, for example, be no difficulty in burning

fuel under a pressure of 4 lbs., or even 10 lbs., on the square inch. The conditions necessary are, sufficiently powerful blowing machinery and a very small calorimeter, obtained by the use of annular stoppers or tompions in the tubes. Of course, we do not advocate the use of such air pressures as we have just named, at least, without due inquiry, because there is some point beyond which the work spent in compressing the air would cost more than the advantage gained. The principal benefit to be obtained would, we believe, be perfect combustion. Mr. Otto has shown that, in his gas engine he can explode mixtures of gas and air so diluted that combustion could not be effected at atmospheric pressure. This is done by compressing the dilute mixture to about 30 lbs. on the square inch. At high furnace temperatures it is indisputable that there is a strong tendency to dissociation manifested, and this is probably one reason why much carbonic monoxide escapes unconsumed with an unusual waste of fuel. Burned under pressure, it is more than probable that the union of the gases would be more readily affected, and one most important result would be, no doubt, that the total quantity of air admitted to the fire might be freely reduced. It is not indeed impossible that complete combustion might be effected with as little as 15 lbs. of air per pound of coal, instead of the 24 lbs. or 25 lbs. now found necessary. It may be laid down, however, that concerning the value of combustion under pressure for steam generation, we are quite in the dark; the only thing that can be said, as far as practice is concerned, being that all the indications are favorable in a high degree to the adoption of the system. Perhaps some of the engineers now interested in forced combustion will push their inquiries a little further. The cost of an experiment would be very small. An ordinary portable boiler with a closed ash-pan, very thick ferrules in the smoke-box ends of the tubes, a Root's blower, and a tank to measure the water pumped into the boiler would suffice. For a very moderate outlay in this way a great deal of very valuable information might be obtained.

BALATA.

From the "Journal of the Society of Arts."

In the *Journal of the Society of Arts* for November 20th, 1863, a list of subjects for premiums was published, amongst which was one "For any new substance or compound which may be employed as a substitute for india-rubber or gutta-percha in the arts and manufactures." This was responded to in the *Journal* for February 26th, and March 4th, 1864, a letter being published in the latter from Sir William Holmes, from British Guiana, advising the despatch to the Society of a box containing samples of balata, both in the fluid or milky, as well as in the dried or coagulated state. In the letter referred to, Sir William Holmes speaks of the small specimen which was exhibited in the International Exhibition of 1862 as attracting a considerable amount of attention, and further says, so far as he could judge, balata was not to be rivaled either by india-rubber or gutta-percha, possessing "much of the elasticity of the one, and the ductility of the other, without the intractability of india-rubber, or the brittleness or friability of gutta-percha." Sir William Holmes further expressed a hope that balata would, ere long, be included as an important item amongst the exports of the colony. Notwithstanding that this was written as far back as 1864, little or nothing has been done since towards making balata a regular article of import; occasional notice has been drawn to it from time to time, and the subject as frequently allowed to drop. As a proof of the truth of Sir William Holmes' statement as to the ductility of balata, it may be mentioned that a sample of that exhibited in the Exhibition of 1862, and presented to the Kew Museum at the close of the Exhibition, is still in a fairly ductile state, and shows no such brittleness as is the case with gutta-percha.

In connection with this subject of the development of balata, Mr. G. S. Jenman, Government Botanist, and Superintendent of the Botanical Gardens in British Guiana, has just drawn up a very exhaustive report, the result of which, it is hoped, will be to bring the substance into a regular commercial channel.

The title of the report is "Balata and the Balata Industry, Forest Laws, &c.," and it commences with a very interesting description of the bullet tree region, including its inhabitants, character of the vegetation, &c. Coming to the immediate subject of the report, Mr. Jenman describes the bullet tree, from the bark of which balata is obtained, as a large forest tree ranging from Jamaica and Trinidad to Venezuela and Guiana. He refers it to *mimusops balata*, and says: "The vernacular name appears to be applied to two species or sub-species which are united by Grisebach, in his 'Flora of the British West Indies.' Young plants of *mimusops globosa*, of Jamaica and Trinidad, growing in the Gardens, seem to be distinct from the Guiana type. The tree grows to a height of 120 feet, and has a large spreading head. The trunk is nearly cylindrical. The bark is about half an inch thick, with deep parallel fissures an inch or so apart. The hard reddish-colored wood is one of the densest in the colony, and is used for all sorts of purposes where great strength and durability are required. The tree is more plentiful in both the eastern and western parts of this colony than in the intermediate region. From the east bank of the Berbice river to the Corentyn is the region of its greatest plentifulness in the colony, but its distribution extends still eastward beyond the Corentyn into Dutch Guiana, where a grant of several hundred thousand acres has recently been acquired by an American firm for collecting balata. The trees are more plentiful in this region in the depths of the forest than near the rivers, hence the creeks form arteries to the balata grounds. Several of the creeks on both sides of the Canje are instances of this. The wood cutters of this district regard the tree as inexhaustible; in the interior of the forest it exists in profusion and abundance, and lies beyond the reach of the balata collectors as they at present conduct their operations. As the trees near at hand become exhausted, they will, no doubt, alter their habits, and make clearings as drying places in the heart of the forest; but now they are under the obligation of returning to the settlements on the creeks with the milk they have collected to dry. Under this necessity they can at most only penetrate about two days' journey, but, so far as they have explored, they report there is no diminution in the abundance of the trees. The forest at this depth, of course, has never been touched by woodcutters, as, for convenience in getting their timber out, they have to confine their operations to the banks of the river and creeks, rarely going in more than a mile or two."

Regarding the character and value of balata, Mr. Jenman says its strength is very great, and as it does not stretch under tension, for special appliances, such as bands for machinery, it is unequaled. It has recently been pronounced by an American firm of manufacturers as "the best gum in the world."

Dr. Hugo Müller, F. R. S., in a report on the substance says: "It seems that balata is by no means neglected, and, in fact, it would find ready purchasers if more of it came to the market; as it is, the supply is very limited, and generally it comes only once a year. It commands a higher price than gutta-percha, and this in itself is a proof of its urefulness. It is used almost in all cases in which gutta-percha is used, but on account of its higher price, only for superior purposes. It seems that balata is treated by the manufacturers simply as a superior kind of gutta-percha, and, therefore, its name disappears when manufactured. Nevertheless, balata is distinctly different from gutta-percha, and this is especially manifested in some of its physical characters; for instance, it is somewhat softer at ordinary temperature, and not so rigid in the cold.

"In one respect balata shows a very marked and important difference from gutta-percha, and that is its behavior under the influence of the atmosphere, whilst gutta-percha, when exposed to light and air, soon becomes altered on the surface, and changed into a brittle resinous substance, into which the whole of the mass is gradually converted in

the course of time. Balata, on the other hand, is but slowly acted upon under these circumstances. The electrical insulating quality of balata is said to be quite equal to that of gutta-percha."

Mr. Jenman says that the collecting of balata is an open and recognized business, is carried on only in Berbice, but he proceeds to show that the greater part of that so collected is not obtained from trees on Government grants, but surreptitiously from Crown lands; and Mr. Jenman further says that much damage is done to the Crown lands by the depredations of collectors, and "that it is desirable, in the interest of the colony, till effective rules are devised for the protection of the forest and the preservation of this valuable wood, that the trade should be discontinued."

The life of the balata collectors is a very hard one. The ground they have to traverse is generally very wet and swampy. In many cases the traveler sinks at every step up to his knees, and this continues for miles, and water often has to be waded through up to the armpits. When the collecting ground is not too far distant, women sometimes accompany the men, and cook or assist in laying out the calabashes, and collecting the milk while the men fell and ring the trees. The collectors connected with a grant sell the milk they collect to the agent on the grant, and never dry it themselves. The price for pure milk is four shillings per gallon, or occasionally a dollar, and for clean well dried balata one shilling per pound. Considering the circumstances of the people who follow it, balata collecting, if pursued with industry, is a paying business. The calling pays better, while the season lasts, than the best mechanical trade; with fair weather, a man can earn from one to five dollars a day at it, and an exceptionally expert collector has been known to make twenty dollars in three days.

The instruments used in collecting the milk are an axe for felling the trees, a cutlass for making the channels in the bark to cause the milk to flow, and two or three gourds in which to collect the milk. The collector commences operations by chipping a piece of the bark from the selected tree, and if the milk runs well he quickly shaves the moss and rough bark from the side he intends to tap, then stooping down with his back to the front of the tree, but on one side of it, he cuts from the base of the tree obliquely upwards towards himself, in the bark, a narrow channel, then moving round the other side, a similar one. These grooves are generally about eighteen inches long; they form an acute angle at the base, just below which a niche is cut in the bark and is slightly lifted with the end of the cutlass, and a calabash inserted by the rim under it. Occasionally a piece of palm or maranta leaf is inserted under the bark, and the calabash is placed on the ground, the leaf conducting the milk into it. The channels are then quickly cut upwards parallel to each other on the opposite sides, about ten inches apart, the operator continuing them as far as he can reach, which is about eight feet from the ground. The milk trickles from cut to cut down this zig-zag line into the calabash beneath. The best collectors cut the bark with much neatness and precision, and do not injure the trees; but little care is usually taken, and the wood is injured with every stroke of the cutlass, the result being that numerous trees are killed, and left standing. Large trees are always tapped on the opposite sides, careful collectors leaving the intervening spaces for subsequent years. It takes from five to ten minutes to cut the channels in each tree, and the milk runs from forty to sixty minutes; at first it forms a little rivulet, but after about twenty or thirty minutes, it only drips. After a little use, the gourds become so coated on the inside with dry balata, that they have to be occasionally soaked in water, when it peels off freely, leaving them perfectly clean again. The yield of a tree varies according to circumstances. If favorable, a tree 15 inches to 20 inches in diameter, bled 8 feet high, will yield 3 pints of milk. Trees are often felled, and then tapped by ringing the bark in parallel transverse lines, at intervals about a foot apart.

To dry the milk, it is poured into shallow wooden trays, the insides of which are previously rubbed over with oil, soap, or grease, to prevent the balata sticking, and the substance is exposed to as much air as possible, and sometimes to the sun. In fine weather it takes two or three days to dry, and in wet weather a

week or more; when it is sufficiently dry to be removed from the boxes, the sheet is thrown over a line, or bar, to drip, and become hard.

A good deal of foreign matter is found in the milk, and Mr. Jenman says adulteration is systematically carried on, and the agents have at all times to be on their guard against it.

The report concludes with a consideration of the damage done to the forests, and some remarks on their better conservation.

ENGINEERING NOTES.

HULL ALEXANDRA DOCK.—This dock, which was inaugurated yesterday week, is 2,300 ft. in length and 1,000 ft. wide, and contains a water area of 46½ acres. This is a very large water space, as compared with any of the older docks at Hull, and the advantages of great width and length are increased by the depth of water, which will be 34 ft. 6 in. The entrance to the dock, which is bell mouthed, has received much attention, and the lock is 550 ft. in length. with a width of 85 ft. There is a swing bridge over the lock for the railway, and there are three pairs of gates for letting vessels in and out. The depth of water on the sill at high water ordinary spring tides is 34 ft. A river bank, which has been made, has a total length of 6,100 ft., and the whole area of the dock estate is 192 acres, of which 152½ acres have been reclaimed from the Humber. With the view of accommodating the largest class of vessels, the Hull and Barnsley Railway and Dock Company has constructed at the north-east corner of the dock two dry docks. One is 500 ft. in length, with an entrance of 60 ft., and the other is 550 ft. long, with a width of 65 ft.

AN experiment is shortly to be made in the conveyance of laden railway trucks between the mainland and the Isle of Wight. The Carrier, a paddle steamer, possessing facilities for receiving cargoes, has been acquired, and it is intended to run this vessel from Langstone Harbor to Brading. A wharf is to be erected at Langstone, and a line of Railway constructed to the water's edge. On the upper deck of the Carrier a double line of rails has been laid from stem to stern, affording accommodation for fourteen ordinary railway trucks. The vessel will come alongside the wharf, where there are steam appliances for running the trucks on the deck direct, and at Brading similar facilities will be provided for unshipping them, and again placing them on the railway. It is also anticipated that in the course of time passenger trains may be carried across the water by similar means. At present considerable difficulty is experienced in conveying merchandise to and from the Isle of Wight, the arrangements necessitating the unloading of goods, which occasions considerable delay. This will be entirely obviated should the experiment prove successful. One of the principal objects of the new system is to facilitate the transit of vegetables, &c., for the Isle of Wight, and it is calculated that at least two hours will be saved by this means. It is stated that trucks could be filled with coal at the pits and conveyed to any station in the Isle of Wight without being unloaded, and the same advantage would apply to furniture vans. After the arrival of the Carrier at Portsmouth from Newhaven a trial was made of her seagoing capabilities between Langstone and Brading with satisfactory results.

IRON AND STEEL NOTES.

EXAMINATION OF CAST STEEL.—We learn from *La Metallurgie* that some interesting studies on the structure of cast steel have been made in the laboratories of Creusot by MM. Osmond and Worth. It was already known that cast-steel consists of a kind of cellular network of a carbide of iron, not easily attacked by acids, inclosing particles of soft iron easily attacked and dissolved. In order to examine this structure more closely, MM. Osmond and Worth prepared some very thin sheets from the samples to be examined, not exceeding two or three hundredths of one millimeter in thickness. These were attached to glass plates by means of Canada balsam, and then exposed to the action of dilute nitric acid, which dissolves out all the soft iron, and leaves the network of carbide in a form convenient for examination. It was found that the distribution of the network was not uniform; groups of carbide cells occur together, with spaces between made up of soft iron. The regularity in the diffusion of the carbide appears to influence the quality of the steel, as that steel which had been most worked was most uniform in structure.

FROM 1862 to 1882 the production of sulphuric acid in Germany has increased from 22,311 tons to 157,961 tons, chiefly obtained from Siegen pyrites. Lump pyrites are burnt in kilns with movable grates, and smalls in Perret or Maletra kilns. The burnt ore from certain mines is afterwards smelted for iron, but the Siegen ore retains too much sulphur to permit of its use in metallurgy. M. Hasenclever seeks to refute the views of Dr. Lunge on the inconvenience of zinc sulphide—blende—in the manufacture of sulphuric acid.

A NEW method of producing alloys of iron or manganese, or iron and manganese, with tin, is described in a patent taken out by Charles Billington and John Newton, who claim the introduction of iron or manganese, or of iron and manganese not previously alloyed, into a bath of molten tin, kept at a suitable temperature, the iron or manganese being connected by wires to a dynamo machine or battery. When put in circuit it is claimed that the current of electricity throws off the iron or manganese of iron and manganese into the tin bath, and produces chemical action between the metals, which causes them to alloy with each other in any desired proportion that can be regulated with great nicety.

A TEMPERATURE of 570 deg. will produce a dark blue color on polished steel, and 590 deg. a pale blue. Oil or grease of any kind will answer for drawing the temper of cutlery. The temper for lancets is obtained at 430 deg. Fahr., axes at 500 deg., swords and watch springs at 530 deg., small saws at 570 deg., and large saws at 590 deg. Copper-colored spots are not produced by tempering, but they may be obtained on the polished surface of steel by immersing the article in a solution of sulphate of copper.

THE CLAPP-GRIFFITHS STEEL PROCESS.—There has been a tendency of late to render smaller users — comparatively speaking — of steel independent of large manufacturers, or, putting it another way, to enable them to become their own manufacturers without having to put up an expensive plant. This tendency is illustrated by several devices for this purpose which have been described in our columns, some of which are certainly meritorious. Amongst these latter, and the latest that has been brought under our notice, is the invention of Messrs. Clapp and Griffiths, of Newport, Mon., which consists in a fixed converter for making soft steel to take the place of wrought iron. The object is to bring the question of steelmaking within the reach of manufacturers having puddling or charcoal iron forges, at a small cost for plant; and as steel is so rapidly replacing iron, this is an important matter to the trade, the members of which in most cases have to purchase steel from the large makers. The converter is a fixed vessel about 5½ ft. outside diameter, by 10 ft high, lined with silica bricks, and is provided with from four to six horizontal tuyeres, having valves at the rear for regulating the blast during the operation of blowing. A very important feature in the invention is the position of these tuyeres, which are placed about 9 inches above the floor of the converter, and only 4 or 5 inches under the surface of the metal when charged. This enables a soft blast of only from 5 to 6 lbs. per square inch to be used as against from 20 to 25 lbs. in an ordinary Bessemer converter. As the slag rises on the metal, it is run off through a slag hole, and when the blow is completed, the steel is tapped, in the same manner as iron is drawn from an ordinary cupola. It is then taken in a ladle, after the addition of ferro-manganese, to the casting pit and run into ingots. The pig-iron is first melted in a cupola and then run into the converter, where the conversion into steel takes from 15 to 20 minutes. It is stated that plant can be erected at a moderate cost to make from 50 tons and upwards per week according to the number of converters at work, the capacity of the converters varying from 1 ton to 3 tons per charge, and the number of charges per day of twelve hours being from twelve to fifteen for each converter. It is claimed that a Clapp-Griffiths plant can be erected at a cost of about one-half of a Siemens plant to produce an equal quantity of steel, and in cases where forges have small blowing engines suitable for the purpose, the cost is considerably less. The process has been worked for some time past by both English and foreign firms with, we understand, every success.

Not having had the opportunity of inspecting the process ourselves, we have, so far, only the statements of those interested in the invention to go upon. Let us at once say that we accept their statements in perfect good faith. Their views have been so modestly advanced as compared with those of some other inventors which have of late been submitted to us that we feel bound to accept them without hesitation or reserve. It is, however, satisfactory to us to be able to produce confirmatory evidence of the merits of the invention perfectly independently of the inventors or their friends, and, indeed, without their knowledge. It so happens that we have just received from the United States two papers upon this process. These papers, which were read before a recent meeting of the American Institute of Mining Engineers, held at New York, were, one by Mr. J. P. Witherow, of Newcastle, Pa., and the other by Mr. R. W. Hunt, of Troy. From these papers it appears that the Clapp-Griffiths process was tried in South Wales two years since with a reasonable amount of success, the operations having been witnessed by the authors of the two papers. The process has since been tried at Pittsburgh with improved machinery, and with results which it will be seen produced a strong impression at the meeting of the institute to which we have referred. It would appear that the process almost completely eliminates the silicon, while leaving untouched the phosphorus, in fact, the percentage of the latter in the steel is higher than that in the pig nearly in proportion to the concentration due to waste in blowing, and the cinder contains only traces of phosphoric acid. As far as regards the mechanical qualities of the steel, it is demonstrated that low silicon permits of high phosphorus and thus it has been suggested that, since the Clapp-Griffiths process has shown its capacity to turn out with special success mild steels, and since it proves that the elimination of phosphorus is not necessary provided the silicon is removed, therefore it partly covers the ground occupied by the basic process. Mr. Hunt, while believing that it cannot make rails or ship plates in competition with the regular Bessemer plants, holds that it can compete with them in small products, even if it does not make an article which they cannot produce. The quality of the metal made by this process in America has been proved, many hundreds of tons of it having been made by Messrs. Oliver Brothers & Phillips of Pittsburgh, and placed on the market in different forms, and consumers have recorded their approval of it. Mr. Witherow states in his paper that it costs 50s. per ton at Pittsburgh to convert pig-iron into puddled bars, but a ton of pig-iron can by the Clapp-Griffiths process be made into steel blooms for 24s. in mills, or about half the cost, while, if the iron is run direct from the blast-furnace into the converter, the cost will only be about 16s. This testimony is borne out also by Mr. Hunt, who believes the converter will be found desirable for existing works whose products in the past have been exclusively wrought iron. In connection with

the technical features, it may be stated that, when the process was first started at Pittsburgh, the best brands of English Bessemer iron were used, and the steel produced so far exceeded all requirements that a lower standard was ventured upon, and a pig was used having a greater proportion of phosphorus. A mixture was tried which gave a metal with about 0.34 per cent. of phosphorus, and this worked so well that Mr. Hunt doubled his proportion of high phosphorus pig, obtaining a steel with 0.54 per cent. phosphorus, and a test piece was bent double when cold, and worked very satisfactorily when hot. As yet it has not been ascertained how much higher the content of the phosphorus in the pig may go, and this appears to be the only question remaining unanswered. The composition of the steel is shown by several analyses in Mr. Hunt's paper; and, taking that ascertained just before the meeting, we find that there was: Carbon, 0.08 per cent; silicon, 0.01; phosphorus, 0.50; manganese, 0.48; and sulphur, 0.09; while the physical tests gave tensile strength about 80,000 lbs., elastic limit 59,000 lbs., elongation 23 to 24 per cent., and reduction of area 36 to 37 per cent., in all cases ½-inch round test pieces 8 inches in length being used. On the whole, the results appear to be most satisfactory, and, judging by them, the question arises as to how far the Clapp-Griffiths may affect other steel-making processes on a large scale. It appears destined to influence the trade by its application in the direction indicated by us at the commencement of our article, and if the results of working on the smaller are corroborated by those produced on the larger scale—should it be so tried—it might go hard with some of our leading works. Many nice questions, however, will have to be settled before that day arrives, and, in the meantime, there is an ample field for the process amongst users of steel in moderate quantities, who may be glad to avail themselves of the advantages it offers.—*Iron.*

RAILWAY NOTES.

THE railways of New South Wales are the property of the Government. The Southern line is in operation from Sydney to Albury, a distance of 386 miles, and will be opened before the end of the present year to Albury, where it joins the line from the Murray to Melbourne. A branch is also being pushed on from Junee to Hay in a westerly direction. The Western line, which crosses the Blue Mountains, is open to Wellington, 248 miles distant from Sydney, and is being extended to Bourke, 504 miles from Sydney, on the river Darling. A suburban line runs to Richmond and Parramatta; while from the important seaport of Newcastle there is a railway communication to Glen Innes, a distance of 399 miles; and to Narrabri, a distance of 320 miles; while numerous other lines are being commenced.

IN a report on a collision which occurred on the 23d May at Boston station, on the Great Northern Railway, Major Marindin says: "This collision was caused by the coupling between the engine tender and the brake van becoming detached upon a falling gradient, immediately the engine began to push these vehicles back towards the passenger train. The evidence appeared to show that the shackle was not properly oiled, and that the coupling being stiff, it was pushed off the hook when the engine began to set back. I think that the best means to adopt in order to prevent the recurrence of an accident from a coupling coming loose would be to fit the draw-bar hooks of all vehicles with a spring or a weighted catch as is done upon the London and North-Western Railway and other lines, and until this is done it is clearly advisable that in the operation of putting empty carriages on to a train the couplings should be screwed up sufficiently to make it impossible for them to become detached as in this case. It would also be well to have a porter in the brake van to make use of the brake power if necessary, except, of course, in cases when the vehicles are fitted with an automatic brake coupled on to the engine and in working order."

ORDNANCE AND NAVAL.

IN the following figures are given, first, the sea-going merchant fleets of all nations, and, second, the steamships of all nations:—Great Britain, 22,500 vessels, 11,200,000 tons; United States, 6,600 vessels, 2,700,000 tons; Norway, 4,200 vessels, 1,500,000 tons; Germany, 3,000 vessels, 1,400,000 tons; France, 2,900 vessels, 1,100,000 tons; Italy, 3,200 vessels, 1,000,000 tons; Russia, 2,300 vessels, 600,000 tons; all nations, 46,000 vessels, 23,000,000 tons. Thus it will be seen at a glance how tremendously England outrivals every other marine Power. Her preponderence is even greater in steam vessels, as appears in this second statement:—All nations, 7,764 steam vessels, 9,232,000 tons; Great Britain, 4,649 steam vessels, 5,919,000 tons; France, 458 steam vessels, 667,000 tons; United States, 422 steam vessels, 601,000, tons; Germany, 420 steam vessels, 476,000 tons; Spain, 282 steam vessels, 305,000 tons; Italy, 135 steam vessels, 166,000 tons; Holland, 127 steam vessels, 155,000 tons; Russia, 194 steam vessels, 149,000 tons.

THE ELECTRIC LIGHT AND GUNS.—Mr. Walter Winans, of Brighton, has applied the incandescent lamp to the front sight of a rifle so as to render it visible in the dusk of evening or when there is insufficient light to take an aim. It consists of a miniature electric lamp fixed near the muzzle of the gun and shielded by a metal screen having a hole in it turned to the shooter, who thus sees a bead of light as the front sight. The current is supplied by a small battery in the stock of the gun. We may add in this connection that the big 14-ton gun in the Inventions Exhibition was brightly illuminated inside by an arc lamp on the occasion of the Society of Arts Conversazione. The lamp was provided by Colonel Maitland, R. A., who devised it for examination purposes. It was manufactured by Messrs. Johnson and Phillips. and supplied by a Holmes-Burke primary battery.

A NEW PROTECTION TO SHIPS.—The long-standing rivalry between heavy ordnance and armor plates is likely to be disposed of in a manner little expected, as a means appears to have been discovered whereby the effects of shot and shell, and even torpedoes, will be effectually neutralized. For some time past, naval architects have ceased to rely solely upon armor for the protection of ships, for, notwithstanding the enormous thickness to which armor plates had attained, they were found to be no match for the artillery that was brought to bear upon them. Steel plates and compound plates were next tried, but to no avail. As a further increase in the thickness of plates—whether of iron or steel, or both combined—was impracticable, owing to the overweighting of vessels with armor, shipbuilders tried the expedient of supplying a second line of defence in the coal bunkers, which were constructed along the sides of ships, especially those parts where the machinery and magazines are located. They certainly, to some extent furnished that second line without overburdening the vessel, for coals have to be carried under any circumstances. But a far more effective protection appears now to have been supplied by the invention of a composition which, besides being efficacious as a protector, possesses the merit of being light, a desideratum much wanted by naval constructors. This composition is a preparation obtained from cocoanut cellulose, which has the remarkable property, when penetrated by shot and shell, or even after the explosion of a torpedo, of closing up as rapidly as it has been perforated, and thus preventing the influx of water into the ship's hold. The very appropriate name of "cofferdam" has been given to the preparation, which, besides being very light, is highly elastic and tenacious. Some important experiments have lately been made with the composition before a French commission at Toulon, which, if everything that is reported concerning them is true, proves the preparation to be destined to solve the armor-plate controversy. The commission submitted the composition to a threefold test—against shot, shell and torpedo. The target was a cofferdam made of a mixture of 14 parts of pulverized cellulose and 1 part of cellulose in fiber. This composition was compressed to a felt-like mass, of which one cubic meter weighed 120 kilograms, or one cubic foot about 8 lbs. A layer of beams 12 centimeters (4⅔ in.) thick represented the side of the ship, behind which there was a layer of cofferdam 60 centimeters (2 feet) thick Against this target a 19 centimeter (7½ in.) solid shot was fired, which penetrated it, taking with it not quite one-fifth of a cubic foot of composition, a very small quantity, considering the size of the shot. But as soon as the shot had passed through the target the cellulose composition closed up again, and so firmly that a strong man was unable to force his arm through the opening made. A box filled with water was then fixed against the aperture, the contents of which ought to have acted in the same way as if the cofferdam had been washed by the sea. It was observed that a few drops of water began to percolate after the lapse of from ten to fifteen minutes, and even after the composition had become well saturated with water, only between three and five pints of water escaped per minute, which could be easily intercepted by pails. As soon as the cellulose had become thoroughly soaked and grown denser, it offered greater resistance to the percolation of water, which finally almost ceased to flow. The experiments with shell gave similar results, the breach made closing automatically. It was also found that the cofferdam was proof against fire. A special experiment was made, in which red-hot coals were placed upon a mass of cofferdam, and covered with the same composition, the result being that the fire went out. The experiments with torpedoes were not so decisive as those with shot or shell. A chest was anchored out at sea, one side of which was lined with cofferdam, a torpedo attached to it, and exploded. The chest floated for a few second and then sank. When fetched up by a diver it was found that the lid had been blown off, but that the cofferdam composition was little injured. The above experiments appear to prove that the material in question possesses the property of automatically closing a leak caused by shot or shell and of protecting a ship to a certain extent against fire. Whether its use will render ships unsinkable remains to be shown, but we understand that, in order to investigate this point thoroughly, further experiments on a larger scale are to be undertaken by the Toulon commission.—*London Morning Post.*

THE NEW NAVAL GUNS.—The Woolwich correspondent of the *Times* writes: The new guns which have been designed to maintain the naval supremacy of Great Britain are in an advanced state, but they have to undergo a course of experiments to settle the range tables and other particulars, and it will probably be the beginning of next year before they are ready for sea. This will, however, be earlier than the ships which are to carry them can be completed, and there will be ample time available for a full and leisurely study of their requirements and capabilities. The first of the four 63-ton steel breech-loaders for Her Majesty's ship Rodney, will be shortly finished, and will be used as an experimental gun, care being taken that it is not damaged in the process by any of the surgical operations to which the experimental guns are occasionally subjected. Although 17 tons lighter than the 80-ton muzzle-loaders on board the Inflexible, the 63-ton gun is expected to surpass the older weapon in its destructive power. It will probably throw a 13½-inch shot, of 1,250 lbs. weight, with a powder charge of about 580 lbs., and the estimated velocity at the muzzle is to be 2,100 ft. per second. The 80-ton gun projectile weighs 1,700 lbs., but the cartridge is but 450 lbs., and the muzzle velocity recorded is 1,600 feet per second. Should the new gun realize expectations, it will penetrate 29 inches of wrought-iron armor at close quarters, and prove too much for 27 inches, even at the liberal fighting range of 1,000 yards. Still more powerful, but not in the same ratio of increase, will be the 110-ton guns now being manufactured for Her Majesty's ship Benbow. There are three of these

guns ordered, one of which will be surrendered for the purpose of scientific experiments, while the other two will be sent on board ship, where, however, they will not be wanted until the midsummer of 1886. The projectile will be 16½ inches diameter, and weigh 1,800 or 2,000 lbs. The powder charge will be the enormous one of 900 lbs., or half the weight of the projectile, supposing this to be 1,800 lbs., on which supposition the velocity may be reckoned at 2,050 feet per second, and its power of penetrating armor at 31½ inches near the muzzle, or 2 inches less at 1,000 yards. The new guns will be greatly superior to the Italian 100-ton guns, which are at present at the head of all the naval artillery in the world, and they are also in advance of the 100-ton guns which are doing duty for England on the fortifications of Malta and Gibralter, although these are larger in the bore by 1½ inch. The substitution of steel for wrought iron admits of heavier charges of powder, and this fact makes all the difference. Two huge sleighs for the proof trials of these and similar guns are being built—the one for use at Woolwich and the other for Shoeburyness, whither both the experimental guns just mentioned will be sent for practice at the sea ranges. To Shoeburyness there is also to be immediately sent the 80-ton gun which has been returned to Woolwich from the Inflexible. The inner tube of the gun is unquestionably cracked but this is regarded as a comparatively small injury, and before it is repaired the gun will be fired with a series of heavy charges at the targets which have been put up at Shoeburyness to represent the Spithead forts. These targets, which are respectively faced with granite, wrought-iron plates, and compound steel , have already been attacked in a course of earlier experiments, and the compound steel has shown to very great advantage. The double barge Magog will, as heretofore, convey the 80-ton gun, but for the 110-ton gun a still larger craft is being built, which is to be called the Gog, and measures 20 feet longer than the Magog.

HEAVY ORDNANCE IN THE UNITED STATES.—At the South Boston Ironworks work is progressing in shrinking a series of steel rings upon a 6-inch breech-loading rifled gun, of the same metal, now in process of manufacture. This gun is one of six of like caliber, and two of 8-inch bore, to form a part of the armament of the new cruisers ordered by the American Congress. These guns are all of the built-up pattern. The gun, when completed, will weigh 11,000 lbs., and carry a 100-lb. projectile, propelled by a powder charge of 50 lbs. The initial velocity of the shell is estimated at 2,200 feet per second, and the effective range of the gun at seven miles. The extreme length of the piece is 193.53 inches, the diameter of the chamber is 7½ inches, and its length, including slope, 37½ inches. The rifling consists of 24 lands and 24 grooves (a land being the raised portion between the groove or indentations) with increasing twist of one turn in 180 calibers at the breech, and in a distance of 134 inches to increase to one turn in 30 calibers. The width of grooves decreases 0.05 inch from the breech to the muzzle end of the bore. The breech mechanism is of the interrupted screw pattern, and in rear of the nose-plate is an asbestos and mutton-suet ring, which serves as a gas check. The foundation of the gun is a steel tube 184 inches in length, which is cast in the rough at the Midvale Steel works in Pennsylvania, and which receives interior and exterior finish at South Boston. It is then ready for the jacket, a cylinder of forged steel large enough for the breech end of the original tube to be inserted in it for about one-third the length of the latter, measuring from the breech forward. This tube is set up on end, and the jacket, whose inside diameter is a few thousandths of an inch smaller than the exterior of the tube, is expanded by heat and then lowered into the tube, after which it is shrunk by jets of cold water, making what is called a cold weld. This jacket extends back of the tube 9.53 inches, to accommodate the breech mechanism. In the same manner, excepting that the tube is disposed horizontally, five steel rings are shrunk upon the jacket, in addition to one jacket-hooking ring, one tube-hooking ring, and three chase rings; and, again, outside of the jacket rings are the trunnion ring, which is screwed on, and the elevating ring. The forward part of the tube, 71 inches from the muzzle, is the only portion of the gun not reinforced. The process of shrinking on the jacket and rings is a very delicate one. The expanding is done entirely by gas, the jacket or ring, as the case may be, being introduced into a network of powerful gas jets, which surround the metal both externally and internally with flame. Having been sufficiently expanded, which requires a temperature of 600°, the jacket ring is slipped over the tube, an hydraulic jack of 100 tons' power pressing the enveloping band home. So well does this jacket perform its work that the joints between the several rings are almost imperceptible, and are often no wider than 0.002 inch. The next process is the shrinking, which is done by cold water, as already mentioned. The shrinkage of the jacket amounts to 0.01 inch, of the jacket rings to 0.24 inch, and of the chase rings from 0.19 to 0.16 inch. While the shrinking is going on, the temperature of the tube is kept down by a stream of water running the entire length inside. In addition to these 6-inch guns there are in process of manufacture at the South Boston works one 12-inch cast-iron breech-loading rifle, one 12-inch cast-iron breech-loader with steel hoops and tube, and one 12-inch cast-iron gun with steel tube and a square steel wire coil, having a suction of 0.15 inch. The first of these three guns has been shipped to Sandy Hook. The length of its rifling is 284 inches, of increased twist, sixty lands and grooves. The diameter of the chamber is 13½ inches, and the length 69 inches. The charge is 150 lbs.; weight of projectile, 800 lbs.; the range of the gun is from 6 to 8 miles. The steel tube of the second gun is from the gun factory of Sir Joseph Whitworth, Manchester. It weighs 6 tons, and is 14 feet long. Its rough boring is now being perfected, and the exterior surface machined. It will then be shrunk into a cast-iron envelope weighing 40 tons, and the

finished gun, with its steel hoops will weigh 53 tons.

BOOK NOTICES

PUBLICATIONS RECEIVED.

NOTIZIE Sulla Agricoltura in Italia. 1885. Rome.

Proceedings of the Institution of Civil Engineers:

No. 2054.—Metropolitan and Metropolitan District Railways. By Benjamin Baker, M. Inst. C. E.

No. 2066.—Various Methods of Traction Applicable to Railways. By Marcel Deprez and Maurice Leblanc.

No. 2080.—The Steel Permanent Way of the London & N.-Western Railway. By Francis Wm. Webb, M. Inst. C. E.

Student Paper—No. 190.—Blasting Rock at Blyth Harbor. By William Kidd, Stud. Inst. C. E.

——— Water Supply. By William Pole, F. R. S.

MONOGRAPHS OF THE UNITED STATES GEOLOGICAL SURVEY.—Vol. VI.—Contributions to the Knowledge of the Older Mesozoic Flora of Virginia. By WILLIAM MORRIS FONTAINE. Washington: Government Printing Office.

Vol. VIII.—Paleontology of the Eureka District. By CHARLES DOOLITTLE WALCOTT.

Only students of Paleontology will regard these books with particular interest.

Vol. VI. is embellished with 54 large tinted plates, which supplement the descriptive text in the most satisfactory manner. The plates are tinted, and the typography of this, as of all the other volumes, is excellent.

Vol. VIII. is devoted to descriptions of the fauna of the Cambrian, Silurian, Devonian, and carboniferous formations.

For the student's benefit the text compares the described features with those of species found in other localities, and already known to Paleontologists.

Reference libraries everywhere are incomplete without these reports The sientists of the next generation will not be properly equipped for geological work without the knowledge of what has been recorded during the present researches.

All students of Natural History know how valuable are good illustrations of the objects with which they wish to familiarize themselves, and students have long since learned to place a high values upon the illustrations of the reports of the United States Geological Survey.

WATER-CLOSETS: A HISTORICAL, MECHANICAL AND SANITARY TREATISE. By GLENN BROWN, Architect, Assoc. Am. Inst. of Architects. New York: The Industrial Publication Company. 1 vol., cloth. Price $1.00.

In this book Mr. Glenn Brown, who is also the author of one of the latest volumes of the Science Series,* has condensed much useful information, of interest alike to the historian, the manufacturer, the sanitarian and the householder. He begins his subject with a historical review, next describes and illustrates some of the first patented mechanical apparatus for the reception and removal of fæcal matter, and finally discusses at length the modern closets—that is, those types which are at present manufactured and sold. The copious illustrations of the book add much to its value, although we can hardly agree with the author's opinion that they are all made in a uniform style.

On page 144 we notice a slight error in enumerating the well-known English siphon and tumbler-tank of Mr. Isaac Shone as one of a number of water-closet tanks. The tank is never used to supply water-closets with flushing water, but is intended to be placed underground to receive all discharges from soil and waste pipes, and to act as a "hydraulic ejector," as the inventor calls it, that is, as a flush tank to the main house-drain.

We also cannot agree with the writer's opinion of some of the most recent water-closets, and think that upon actual experience in using some of these in his architectural work—and which he evidently did not have when writing his book—he will find it necessary to greatly modify his opinion. In one instance, particularly, his drawing of the closet is accurate, and his description of the same closet faulty and incorrect.

* "Healthy Foundations for Houses." By Glenn Brown, Architect. Van Nostrand's Science Series, Vol. 80.

MISCELLANEOUS.

ALARM FOR ICEBERGS.—The recent collision of the steamer Baltic with an iceberg and the delays and narrow escapes from accidents on the part of other vessels on the Atlantic, has stimulated the wits of American inventors, and numerous suggestions and experiments have followed. Prof. Alexander Graham Bell, and Mr. Frank Della Torre, have been experimenting in the Chesapeake Bay with a speaking trumpet attached to the muzzle of a musket and judging of the proximity of objects by means of an echo. The results of the experiments are of a satisfactory nature and may lead to some means for a more practical application of such devices. When the gun was aimed at passing vessels clear echoes were returned up to a distance of a mile, and the interval between the report and the echo would serve as a basis for estimating the distance in the night. A small steam tug, approaching the vessel bow on, produced an echo in answer to the discharge of the gun when at a distance of one-fourth of a mile, although the echo did not have the clearness of those sent back from sailing vessels. The ripples and waves on the surface also gave continuous echoes like the rolling of distant thunder, and the experiments were more satisfactory in calm than in rough weather. As the motion of the water in the open sea is always considerable in comparison with the land-locked Chesapeake Bay, and the large bulk of a steamer would, in itself, reflect sounds, it is probable that in its present form the utility of this ingenious device is, to say the least, unproven; but some application of the principles governing the reflection of sound appears to be the most feasible manner of giving warning of the approach of icebergs and even of other vessels.

THE MECHANICAL TELEPHONE.—A new form of mechanical telephone was shown in operation recently between No. 4 Ludgate-circus and Chancery Lane, London. The apparatus is the device of Messrs. A. A. Knudson and T. G. Ellsworth, and is a modification of the string telephone, with wire for the string (as employed by Mr. W. J. Millar, of Glasgow, some years ago), and plaited willow chips for the diaphragm or tympan. The plaited willow tympan is found to give very good effects. It is shielded by a wooden cover or mouthpiece. The call consists in tapping this wooden cover. In the recent trials the ticking of a watch was heard distinctly across Ludgate-circus., and whispering all the way from Chancery Lane.

AN INDESTRUCTIBLE, SELF-DEPOLARIZING AND CONSTANT EARTH-CONNECTION.—By JUSTIN MALISZ.—A good earth-connection is of great importance for telegraphic purposes, and in the protection of buildings against lightning. The author, after drawing attention to the defects of ordinary earth-plates, describes his method of preparing an earth-connection which conducts well, is self-depolarizing, and has its junction with the line easily accessible.

A hole about 2 meters (6½ feet) deep, and about 1 meter square, is dug in the ground, and a layer of coke laid on the bottom, and firmly stamped down. A tube with its lower end resting on the coke is placed against one face of the hole, and filled tightly with coke almost to the top. The hole is then filled up, and the earth rammed well round the tube, the upper part of which projects above the surface of the ground. A lump of coke or retort carbon, to which a copper band is rigidly attached by soldering or pouring molten lead over it, is put in the tube, and a final layer of coke pressed firmly around it. The copper band projects from the top of the tube, and to this band the line, or the lightning conductor is soldered.

By this arrangement a constant earth-connection is obtained, as coke has a uniform conducting power under all circumstances, and its large surface ensures for the current an easy path to earth. The column of coke in the tube not only enables the junction between earth and line to be permanently above the ground and easy of access, but it has a depolarizing action on the bottom layer of coke, thereby preventing the prejudicial effect of a counter electromotive force, which in so many cases renders an ordinary earth-plate useless.

The author considers that the coke, which, in the upper part of the tube is in contact with the air, condenses oxygen, and can likewise absorb and oxidize the hydrogen formed by the current. The gases resulting from polarization rise through the column of coke in the tube, and are dissipated in the air, and this column in virtue of its power of condensation plays an important part in the depolarization of this form of earth-connection.

CITY WATER WORKS SYSTEM UNDER PRESSURE.—At present these consist of twelve miles of street mains, supplying 1,500 consumers, or 6 per cent. of the population. There are 139 hydrants, 9 free spills, and 6 drinking and 1 public fountain. The cost to January 1st, 1885, has been about $300,000, while the annual income is now $15,000 per annum. While supplying 1,500 consumers, there have been occasions, during very hot weather in August, when for some hours, the draft on these pipes has been at *the rate of* 1,000 *gallons per head per day*. Street watering accounts for some 100,000 gallons daily; fountains for a considerable amount besides, *but the great bulk of this flow goes for irrigation*. The character of City Creek water is such, Mr. Ottinger informs me, that meters cannot be used at all, owing to quick corrosion, and so great is the effect of the sulphuric acid in this water upon brass, that unless the stop gates are moved at least once every 40 days, they become set and require to be taken out and relieved, so as to be worked at all. It is evident that no matter what other methods may be adopted for conducting water for irrigation, a very large amount will always pass through these pipes and be devoted to that purpose, but it is presumable that some method will be adopted to get payment therefor. When the time arrives, as it will, that the waters of Utah Lake shall be brought to the city, *for household purposes*, by covered conduit or pipe, all these difficulties will be obviated.—*Abstract of Report of C. S. Stevenson.*

AT a recent meeting of the Berlin Physical Society, Dr. Kalischer, described a new secondary battery, intended to overcome the disadvantage of the usual accumulators—namely, that the sheet of lead used as anode was very soon destroyed. This object he is said to have attained by adopting a very concentrated solution of nitrate of lead as electrolyte, and iron as anode. The iron, on being immersed in the solution of lead, became passive, and resisted every corroding effect of the fluid; in other respects the peroxide of lead on the electric charge became deposited at the anode as a very firm cohorent mass, enveloping and protecting the iron on all sides. The charge was continued till the greater part of the nitrate of lead was decomposed, a condition which was marked by the occurrence of a greater development of gas at the anode. At the beginning of the charge all development of gas must be avoided, as otherwise the peroxide of lead, or, more correctly, the hydrate of peroxide of lead, became covered with bubbles. As cathode a sheet of lead was used, but it was attended by two disadvantages. In the first place, the lead during the charge, separated itself at the cathode into long crystal threads, which soon passed through the fluid and produced short closing (of the current). In the second place, the nitrate acid, which remained in the fluid after the separation of the lead, acted very powerfully on the sheet of lead. Both disadvantages Dr. Kalischer avoided by amalgamizing the cathode. This accumulator of iron, concentrated solution of nitrate of lead, and amalgamized lead yielded, after the electric charge, which could be carried out without any special preparations, a current of about 2 volts; after about six hours' discharge, however, the electromotive force sank to 1.7 volt, but, on the battery being left to itself for twenty-four hours, it became a little increased. According to the measurements

hitherto taken, the functions of this accumulator were satisfactory. An attempt to substitute sulphuric manganese for nitric lead in this battery did not answer the purpose, as the peroxide of manganese separated itself, not in a continuous layer, but in loose scales.

ANTI-FOULING COMPOSITION.—A competitive trial of some interest has recently been carried out by the naval authorities in order to test the comparative values of two anti-fouling compositions. They are respectively the inventions of Lieutenant-Colonel Crease and Mr. Sims. The trials have extended over a considerable period of time, having been commenced on the Mercury in 1882. In that year the port bow and starboard quarter of the Mercury received three coats of Colonel Crease's protective composition and two coats of his anti-fouling paint. At the same time the starboard bow and port quarter of the same vessel received two coats of Mr. Sims' protective composition and three coats of his anti-fouling mixture. There were thus five coats on the whole of the ship. The difference in cost is said to have been £50 less on the part allotted to Colonel Crease. Upon the ship being docked at the end of twelve months it was found that the part covered by Colonel Crease was generally in good order, whereas the Sims division of the ship required repainting. This was in January, 1883. It was then determined to have another trial, and the Sims division having been scraped clear, two coats of both the protective and anti-fouling compositions were laid on. The Crease part of the bottom was washed only and one coat of protective and one of anti-fouling paints were laid on. The difference in cost this time was £70. After about nine months the ship was docked again, when the results were found to be somewhat similar to those above described. A third trial was then decided upon, and a coat of varnish was applied over the Sims protective composition before the anti-fouling mixture was laid on. The final results appear by the reports to be decisively in favor of Colonel Crease's method.

PROTECTING WOODEN BUILDINGS.—A very simple method of rendering wood factory buildings of greater resistance to fire, consists in filling the spaces between the studding with a grout made of sand, lime, and a large proportion of sawdust, mixed with sufficient water to flow slowly; it becomes quite hard, is a poor conductor of heat, and will not ignite although it is charred by exposure to an intense fire. This applies to a building already constructed, where it would be a difficult task to remove the sheathing, or lath and plaster already on the inside walls. Where the studding is already exposed on the inner side, the space is frequently filled with brick, masonry, or large tiles made for such purposes. A new material made for such purposes in America is called terra-cotta lumber, and is composed of top clay, which overlies the firebrick clay, mixed with equal or double quantities of sawdust. Every vestige of the sawdust disappears in firing, leaving the tiles very porous. Its use is not limited to filling walls, but it is applied to other purposes of construction where refractory materials are desired, as for short joists between iron floor beams, roofs, coverings to iron columns and beams, sheathings for internally fired boilers, and steam pipes. Small cylinders of this material are arranged with suitable coverings, filled with petroleum, and used for torches. Nails and screws can be driven into it, and it can be cut to dimension with edge tools as desired.

A NEW PROCESS OF BLEACHING.—Mr. Charles Toppan, the American chemist who has been identified with many discoveries in the later derivatives of petroleum for surgical purposes, as cerates, petroleum jellies, and certain antiseptic preparations which are widely used in hospitals and in private practice, has recently been applying, on a large scale, a certain solvent of vegetable gums, which is composed mainly of petroleum products, for the purpose of bleaching cotton goods. At the bleachery devoted to this process, a number of tons of goods are finished daily, and the results are of the most favorable nature, the luster of the goods being excellent, while the action of the solvent being limited to the removal of the gummy matter, the strength of the fabric is not impaired, and the loss in weight averaging ½ oz. to the pound. It has also been applied in an experimental manner to numerous fibrous stalks, as flax, ramie, china grass, sisal, esparto grass, certain of the cactus family, and even the stalks of the cotton plant. In the manner of its application the material is boiled in the solvent, and afterwards treated with the "chemic," or mixture of chloride of lime and acid, in the usual manner. With the thick stalks mentioned above, a certain amount of attrition is necessary during the final washing. It has also been used to deglutinize silk cocoons, the principal value of its application being in its use on perforated cocoons, where in leaving the chrysalis for the moth state, the grub cuts many of the filaments and destroys the silk for reeling purposes. Reeling of silk is commercially successful only in tropical countries where labor is very cheap, but the processes of spinning damaged cocoons is akin to those carried on in the spinning of worsted yarns, and carried on in countries where organized mechanical handicraft may be obtained.

THE New British Iron Company, which, bye-the-bye, is one of the oldest iron manufacturing companies in this country, has awoke to the fact that the printing machine may in various ways facilitate the communication from manufacturer to consumer of information, and that the latter will not hunt up for himself when there are so many ready to supply it. Old reputations are not sufficient to make new consumers run after those possessing them, and hence, the New British Iron Company has just published a well-executed catalogue of its manufactures in iron and steel. This company has always produced first-class irons, and its brands, Lion, Corngreaves, and Ruabon, are known to thousands who do not know to whom they belong. The company now makes steel by the open-hearth process in six different grades, and no doubt will acquire the high reputation which it has held for iron manufacture. The New Iron Company was established in 1825.

A PAPER on "Zinc in drinking water" is given in the *Journal of the American Chemical Society*, by Dr. F. P. Venable. It has long been known that zinc dissolves in water, and that soft water, such as rain water, dissolves it more easily than hard water. Water containing carbonic acid is specially able to dissolve it. The use of galvanized iron for pipes and tanks being so much on the increase, the subject becomes more and more important, and it is desirable to ascertain, as for as possible, to what extent solution of the zinc coating takes place, and how far water contaminated by zinc is injurious to health. The author quotes several investigators as to the latter point, the evidence being to some extent conflicting, but giving a very decided balance on the one side of the view that such water is considerably injurious. Investigations made on behalf of the French Government resulted in the prohibition by the Ministry of Marine of the use of galvanized iron tanks on board of men-of-war. Professor Heaton has given an analysis of a spring water, with a further analysis of the same water after it had traveled through half a mile of galvanized iron pipe. It had taken up 6.41 grains of zinc carbonate per gallon. Dr. Venable gives the results of an observation of his own, where spring water passed through 200 yards of galvanized iron pipes to a house, and took up 4.29 grains of zinc carbonate per gallon. It seems pretty clear that drinking water should not be allowed to come in contact with zinc.

RAINFALL OF SALT LAKE CITY.—The following table of rainfall is compiled from records kept at Fort Douglas from 1865 to 1875, and from that date to January 1st, 1885, from the U. S. Signal Office in this city:

TABLE No. 1.

Years.	Ins. Rain.	Years.	Ins. Rain.
1865	15.51	1875	21.07
1866	22.29	1876	18.31
1867	26.14	1877	14.52
1868	17.25	1878	17.86
1869	22 32	1879	13.11
1870	20.96	1880	10.94
1871	23.12	1881	16.88
1872	18.12	1882	16.00
1873	17 37	1883	14.24
1874	19.55	1884	17.52
Mean of 20 years			18.15

From this table we learn, that, for twenty years past, the average annual rainfall in this valley has been 18.15 inches: and the smallest amount recorded, 10.94 inches, in the year 1880. From the same source it is also ascertained, that the average summer rainfall, or rather that which occurs between the middle of May, and middle of September, has been 3.31 inches. Since we cannot predicate the available water, upon any other than the delivery of a very dry season, that of 1880, the driest known for 20 years, has been adopted as the basis for obtaining the least fall of rain upon the various drainage basins.

WITH the view of improving the harbor, the Ayr Harbor trustees have resolved to pile the north wall for a distance of 300 ft. at a cost of £2,000.

FIREPROOF DOORS.—The most efficient fireproof doors are wood covered with tinned iron. The door is made of two thicknesses of tongued and grooved boards, crossing each other diagonally and thoroughly nailed together. The sheets of tin are bent over at the edges, forming locked joints as in a tinned roof; it is important that the edges, as well as the sides of the door, be covered, as its resistance to heat lies in the fact that the fire cannot burn the wood thus protected against exposure to the air, nor can it warp it, as is the case with an iron fire-door subjected to slight heat. If a fireproof door is hung on hinges, especial care must be taken to insure their security by fastening them to the door by means of bolts, rather than screws, and connecting them to the wall in an equally secure manner. The latches should be selected with a view to durability, as such a heavy door is apt to be destructive of weak latches. Where the position of the doorway permits sliding doors, it is preferable to have them on tracks, care being taken that the cleats be placed on the floor each side of the doorway, so as to secure the door at its lower corners when shut. In the Boston Storage Warehouse, U. S., there are a large number of such doors in the fire walls, arranged to close an electric circuit when they are all shut, and the fact is recorded on the paper dial of the watchmen's clock at certain intervals. Fireproof doors are frequently arranged to close in advance of a fire by means of the yielding of the alloy fusible at 160 deg. Fahr. The track upon which such a door is hung inclines about 1 foot in 8 feet, and the door kept from closing by means of a round stick about 1 inch in diameter, which reaches from one edge of the door to the opposite side of the door frame. At the middle the stick is cut in two diagonally, and a ferrule made of two pieces of thin copper soldered together longitudinally with the fusible alloy, covers the joint in the stick. When this ferrule is exposed to a temperature of 160 deg. Fahr., its yielding causes the ferrule to split open, and the stick separates into pieces and allows the door to shut. In order that the stick shall not fall in the way of the door, and that the door may be shut at any time, the stick is connected to the top of of door frame by small chains near to each end. This simple device was designed by Mr. Lewis T. Downes, president of the What Cheer Mutual Insurance Company. Another method of utilizing this fusible alloy to close fireproof doors and shutters, is by means of a wire extending around the room, and containing in various places links made of two pieces of brass soldered together. When the solder melts and allows the two pieces of brass to separate, the wire allows the shutter or door to close. Mr. Frederick Grinnell has improved the ordinary link by cutting a slot in one of the pieces of brass, and laying a short bit of wire therein, when they are being soldered together; the solder flowing around this wire presents a resistance in three planes, in place of the ordinary joint, which may be imperfect and lies in a single plane, concealed by the sheet brass so as to prevent inspection. Formerly, solid links of fusible alloy were used, but the metal has so little resilience that it is apt to gradually lengthen, and finally break at some inopportune time.